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Sir J. J. Thomson, O. M., F.R.S.

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On the 18th December of this year we celebrate the centenary of the birth of one of the most remarkable physicists of our time. As a mathematician, clumsy with his hands, he was elected to the Cavendish Professorship of experimental physics at the age of 28 and became an outstandingly skilled experimentalist. He might have been classed as a narrow specialist in the field of electric discharge in gases, yet he rode the crest of the wave that brought in all the crowded advances which started modern physics. Many people thought of him as a typical example of the absent-minded professor, full of strange foibles and with his mind voyaging away in realms of abstract thought — yet he proved wise and skilled as an administrator in his Mastership of Trinity College, and he would have made a first-class financier or business man. Most important of all, and perhaps nearer to his heart than his own career and scientific work, was his part in building up the great school of physics at the Cavendish Laboratory in the University of Cambridge. An appendix to his autobiographical *Recollections and Reflections* records austerely, but proudly, the lists of his students who became Fellows of the Royal Society and those who became in turn professors of physics in British universities and throughout the world. A large and distinguished portion of the physicists working today are pupils once or twice removed of men who were taught by Sir J. J. THOMSON or who worked with him in those epoch-making years.

JOSEPH JOHN THOMPSON was born in 1856 at Cheetham Hill, a suburb of Manchester. His father carried on there the family business of bookseller and publisher which had been established some two generations earlier, spe-

cialising in rare and antique books. Thus the boy grew up in an atmosphere of books, although there was little if any indication of any scientific interest or ability which he might have inherited. After an uneventful childhood the first step leading to his scientific career was taken almost by accident. It was intended that he should become an engineer, and the only way of doing this then was to become an apprentice to a large engineering firm. The firm selected had a long waiting list, and to fill in a couple of years he was sent, at the age of 14, to Owens College, now the University of Manchester — his admission at such an early age being frowned upon, for soon afterwards the authorities resolved that in future pupils should not be taken below 16 years old.

Owens College, at that time a poor and cramped building, had nevertheless a staff of the greatest brilliance and it was here, perhaps more than any other institution in the world, where a schoolboy could find a first-class scientific and mathematical education. Amongst the teachers there were OSBORNE REYNOLDS who taught THOMSON's main subject of engineering, BALFOUR STEWART for Physics, ROSCOE for Chemistry, and THOMAS BARKER for mathematics. All of them but the last became famous for their original scientific work, and BARKER, though he never published a paper, was a most brilliant if unorthodox teacher. He taught THOMSON quaternions—then the « obvious » key to all mathematical physics—before the boy had learnt Cartesian geometry. STEWART considered the boy as his best and most promising pupil; under his influence the young THOMSON wrote his first scientific paper on a question concerning the transformation of energy from one form to another. His experimental work in the laboratory nearly had a tragic outcome when the explosion of a glass vessel almost caused him to lose his sight, but happily there was no permanent injury.

Two years after he entered Owens College, THOMSON's father died leaving his mother and a younger brother, but his abilities were making themselves felt and the future physicist won a number of scholarships in rapid succession and was able to complete his studies there. Towards the end of his time at the College, at the age of 20, he made his first contribution to *Proceedings of the Royal Society* with an experimental paper on contact electricity of insulators; it was communicated by his teacher, BALFOUR STEWART, and made acknowledgments to him and also to Dr. (later Sir ARTHUR) SCHUSTER who became one of his life-long colleagues and friends.

Now that the had shown so much promise in mathematics and in physics it seemed better to leave his own and his late father's intentions of pursuing a career with the chosen engineering firm of locomotive manufacturers, and BARKER strongly advised the lad to try for a scholarship at Cambridge. He was eventually successful and entered Trinity College as a Minor Scholar in 1876, never to wholly leave that College or the University again till the end of his days. The Cambridge Mathematical Tripos was, at that time, a most

powerful institution, retaining all the glory and stimulus it had from the days of NEWTON, but physics, and particularly experimental physics was quite a novelty, not long past the embryonic stage and a subject principally for post-graduate students who had already proved their worth as mathematicians.

THOMSON attended the College mathematical lectures given by THOMAS DALE, J. W. L. GLAISHER and W. D. NIVEN, also like so many other distinguished mathematicians he « coached » with that great teacher, ROUTH. During his undergraduate career he also attended the lectures of Professors ADAMS, CAYLEY and STOKES, the last of which he found clearest and most enjoyable of all. Throughout these years then his training was predominantly that of a pure mathematician, together with those parts of physical science that were mathematical in their content or could be demonstrated during lectures with the minimum of equipment and with no possibility of allowing the student to work with apparatus for himself. His interest in the extra-curricular activities and sports was normal and unspectacular; like many of his contemporaries his chief excercise was the Cambridge « grinds », long walks sometimes extending as far as Royston, 20 km to the south.

In January 1880 he took the Tripos Examination, suffering badly from insomnia during the last five days and relieving the strain by the device of having his hair shampooed between each of the morning and afternoon papers. He was not reckoned to be a good candidate in the strenuous competitive examination, but nevertheless he came out as second Wrangler (Sir JOSEPH) LARMOR being Senior Wrangler and top of that list. Immediately after the examination, having already published two papers on pure mathematics during his undergraduate career, he set himself more resolutely towards physics by entering the Cavendish Laboratory and by preparing a thesis for the fellowship of Trinity College. This thesis had probably first come into his mind while he was still at Owens College but it had necessarily remained dormant during his undergraduate study. The chief content of it was the application of mathematical methods to the study of physical problems involving the transformation of energy; first appearing as his thesis, it was later made the basis for two papers in the *Philosophical Transactions of the Royal Society*, for his book *The Application of Dynamics to Physics and Chemistry* and for several other writings.

It is a matter for some comment that THOMSON was successful at his first attempt in gaining the Trinity fellowship; it was followed swiftly by minor disappointment, for he was an unsucce sful candidate for the new professorship in applied mathematics at Owens College — his mentor, SCHUSTER, who was already working there was elected. For the next four years much of J. J. THOMSON's time was spent in teaching mathematics to undergraduates, first privately, then as a College Lecturer, lastly as a University Lecturer. In 1882 he gained the Adams Prize (established to commemorate the discoverer of

Neptune), his essay being on *A General Investigation of the action upon each other of two closed vortices in a perfect incompressible fluid*, a subject being in keeping with his mathematical bent, but now tending towards some thought on the vortex atom that was then a central feature of a theory of matter.

During these four years, from his degree onwards, THOMSON was also working in the Cavendish Laboratory where he was later to spend the most active part of his life, and we must digress a little to consider the history, until then, of the laboratory that became such a central feature of his work. When one thinks of the present state of science it is hard to realise that until about a century ago there was no such thing as a public university laboratory. Several scientists had possessed their own, relatively small collections of instruments and the national institutions such as the Royal Society of London, the Accademia del Cimento, and the Paris Académie des Sciences had made large collections of apparatus used in their researches and experiments. Before the middle of the nineteenth century the scientists themselves were either gentlemen amateurs of independant means, or they were professional men — medical practitioners or university professors or teachers whose interest happened to become experimental. Such people could buy any instruments needed from the large numbers of specialist mechanics who also made sundials, telescopes and other devices of more general application. By about 1850 several forces were at work, making instruments more varied, more complicated, and more costly, and the advancing front of science and its industrial applications was leading more people to take a practical interest in experimental work.

By about 1870, Chemical Laboratories were flourishing at many universities and it was being conceded that some experimental facilities might be useful in other disciplines of science, and that apparatus was too expensive for any one person to buy a sizeable collection and too complex for its use to be learnt efficiently without formal instruction. Physical laboratories came into being elsewhere in Britain, the first being that of WILLIAM THOMSON (Lord KELVIN) in Glasgow, and later examples at University College and King's College in London and at the University of Oxford. Cambridge was slow in following suit, partly because its Newtonian bias towards the mathematical approach led to a lack of sympathy with « time-wasting » experiments, and partly for the « usual Cambridge reason » (as J. J. THOMSON later called it) — the lack of funds.

In 1870 Cambridge at last moved, thanks to a benefaction from the Chancellor of the University, the seventh Duke of Devonshire, descendant of the great HENRY CAVENDISH. In the following year JAMES CLERK MAXWELL was appointed as the first Cavendish Professor, and he immediately set about the vital task of planning the laboratory which was to be built. He made a thorough study of all the laboratories then in existence and made himself familiar with the latest devices and pieces of apparatus. As a skilled experi-

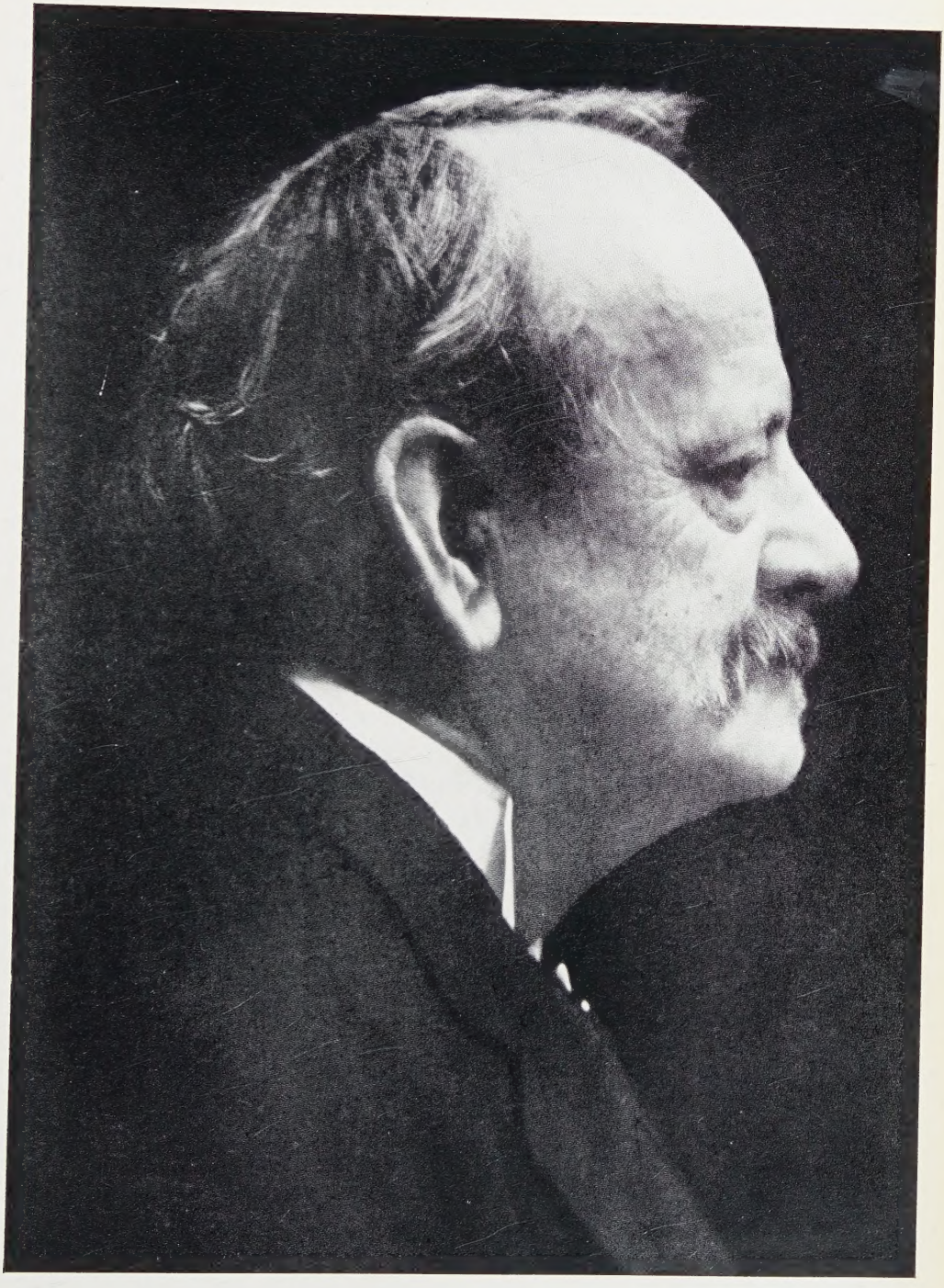
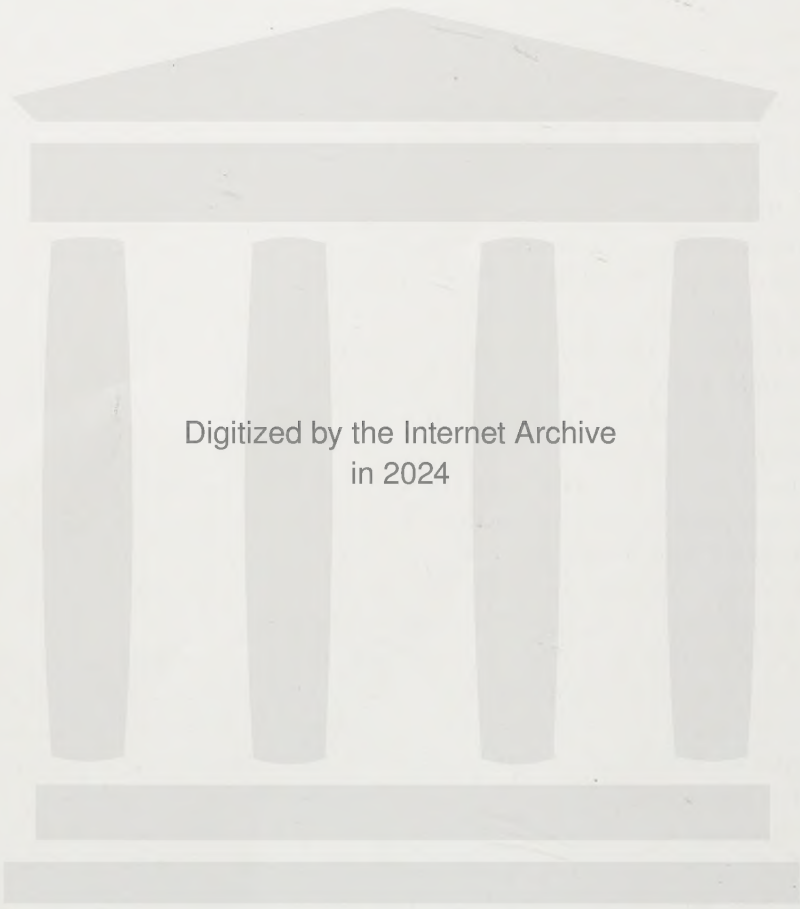


Fig. 1. – Sir J. J. Thomson, O.M., F.R.S. - 1856-1940.



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mentalist he was in an excellent position for such work, and he designed the building, probably for the very first time, as a place particularly suited for physical experiments. Many of the devices and plans which he introduced have now become standard and are still used in physical laboratories. The Cavendish Laboratory was opened in 1874, and MAXWELL continued there (for much of the time as a sick man, working only at an edition of the electrical papers of HENRY CAVENDISH) until his death in 1879 — a year before J. J. THOMSON took his degree.

In spite of the difficulty of finding a suitable successor to MAXWELL, the University decided that the Professorship should be continued, and eventually they managed to persuade Lord RAYLEIGH to accept it, though he stipulated that he would not be willing to hold it for more than five years — he had his own very fine private laboratory (which is still preserved in Terling, Essex) and did not want to tie himself down. Lord RAYLEIGH took the view that the Cavendish was a laboratory which should act in the national interest, rather than as a purely educational device, and he channelled much of the graduate labour force into experiments involving considerable routine labour and observation. The main lines of investigation were directed towards a more accurate knowledge of the fundamental electrical units which were then becoming of considerable practical interest in the rapidly expanding electrical industries of the country.

For four years THOMSON worked in the laboratory under RAYLEIGH. At first he attempted to detect the existence of effects which should have followed from Maxwell's theory that changes in electric forces in a dielectric produce magnetic forces. Results were not conclusive, and at RAYLEIGH's suggestion he next tackled the problem of electrostatic effects between the primary and secondary of an induction coil. Afterwards, again at RAYLEIGH's suggestion, he started a much more fundamental and difficult research on the ratio of electrostatic and electromagnetic units — a ratio which Maxwell's theory suggests should be equal to the velocity of light. RAYLEIGH had himself already constructed apparatus and begun to plan the research, but, as he said in later years, « Thomson rather ran away with it ». The experimental research resulted in a value about 1% lower than that found by other workers, and THOMSON was not satisfied by it, coming back to the problem some seven years later and then, in collaboration with G. F. C. SEARLE, detecting the sources of error.

It must be emphasized that throughout this period THOMSON was working primarily as a mathematician, and his unsuccessful application for the Chair in Applied Mathematics at Owens College shows that he was considering his career along this line too. His interest in physics was centred on the theory of electricity and magnetism, where he had been much stimulated by the work of MAXWELL, and it is clear from his earlier work at the Cavendish that

this was the direction in which he wanted to go. Although the researches to which he was directed by RAYLEIGH may have given him much useful acquaintance with experimental apparatus, they can not have been much to his taste. If RAYLEIGH had not been firm in retiring at the end of his stipulated five years there is little doubt that THOMSON would have tired of these quantitative determinations and retreated into his mathematical investigations.

At the end of 1884 the University was faced with the need to find a new Cavendish Professor. Lord KELVIN, already approached twice before, in preference to MAXWELL and again to RAYLEIGH, once more decided not to leave Glasgow. It was expected that one of the two experienced demonstrators at the laboratory would be chosen—R. T. GLAZEBROOK or W. N. SHAW—the former became eventually head of the National Physical Laboratory, and the latter was Director of the Meteorological Office. But to the immense surprise of all but the electors, J. J. THOMSON was elected at the age of 28. He had recently been in the academic public eye, having just been added to the ranks of Fellowship of the Royal Society, but he was justifiably regarded as a mathematician rather than an experimental physicist. Senior members of the University expressed the opinion quite freely that things had come to a pretty pass when mere boys were made professors, and THOMSON himself admits,

> « I had sent in my name as a candidate without dreaming that I should be elected, and without serious consideration of the work and responsibility involved. When after my election I went into these, I was dismayed. I felt like a fisherman who with light tackle had casually cast a line into an unlikely spot and hooked a fish much too heavy for him to land ».

Such was the first great stroke of fate that was to decide the scientific work of this man.

Our knowledge of subsequent events makes it easy for us to congratulate THOMSON's electors on the wisdom of their choice and their sagacity, now even more uncommon, in not considering the fire of youth as an automatic disqualification for a responsible post. It must however be admitted that in 1884 the appointment did little to help the shaky prestige of the Cavendish Laboratory. Experimentation was still regarded as a waste of time for people who might have been making a real scholarly contribution in mathematics. In his day, MAXWELL had been regarded as a very minor professor in a very remote and specialised department of knowledge. Lord RAYLEIGH, himself a man of high prestige, had found the laboratory of little importance educationally and regarded it principally as a source of cheap labour for a National Physical Laboratory (which was not yet then in esixtence). The young Professor THOMSON was not in a strong position and it took him ten years to

consolidate the laboratory and his own position; during this decade there was slight decline and recovery, but if the shade of great things to come was already making itself felt, its influence was so slight as to be undiscernable.

Although then the first ten years of THOMSON's work at the Cavendish Laboratory must be accounted as relatively unspectacular, one may discern during this time two events of crucial importance to his later work; his choice of a field of specialised study and his marriage. It so happens that marriage was for him not only an event of personal significance but it influenced his life in Cambridge because of the peculiar structure of society there at that time. When he was elected to the Chair there was still great controversy about the admission of women to the university lectures and degrees, and men were required to vacate their college fellowships on marriage. It was strongly felt that women would be a great distraction to the proper work of scholars, and THOMSON may be said to have proved this point for in 1889 he became engaged to one of the first of his women research students — a Miss ROSE PAGET, daughter of Sir GEORGE PAGET, Regius Professor of Physics in the University. She had been working on the problem of soap films thrown into stationary vibrations by the action of sound waves; it was apparently quite a trivial piece of work, but the Professor gave her a great deal of special supervision until, one day, both came downstairs rather flushed and the research was never continued. They were married in 1890 and became one of the very few young married couples in senior academic circles of Cambridge. As such they did much entertaining and their house became a center for the social life of the scientific community. A few years later, in 1893, the Cavendish Physical Society was founded as an informal discussion group, and Mrs. THOMSON ably supported her husband in organising the tea and buns which prepared the way for the more serious business of the day. The social effect was undoubtedly to help the Cavendish become a very pleasant and well-knit group, achieving a unity which it had not possessed before and might never have attained as a simple collection of research workers, students, and their professor.

For the first few years the research going on in the laboratory was spread over the whole field of physics, its miscellaneous character being inherited by THOMSON and maintained through inertia. THOMSON himself continued the investigations, largely in mathematical physics, that he had already started before coming to the Cavendish, but gradually one line of study began to occupy his attention above all others. In 1893 he published a book, *Recent Researches in Electricity and Magnetism*, which was intended as a sequel to MAXWELL's treatise on *Electricity and Magnetism*, bringing the subject up to date and summarising the state of knowledge. The most original part of THOMSON's book was undoubtedly a chapter on the discharge of electricity through gases — the first detailed account of this subject in English. Furthermore, THOMSON was still much inspired by MAXWELL's work, both on the

kinetic theory of gases and on electromagnetic theory — the latter having reached general acceptance and popularity by the confirmation provided by HERTZ's work on electric waves, published in 1887 just previously. In a way THOMSON followed the trend of his time in turning towards the study of all electrical phenomena in gases. It was apparent that the properties of gases are simpler than those of other forms of matter, and more amenable to mathematical treatment as provided by the kinetic theory. THOMSON followed the half-expressed lead of MAXWELL in thinking it likely that the central problems of the theory of electricity and magnetism could best be tackled by further experimental investigation of gases, and beginning his great work at the Cavendish Laboratory, he devoted himself to these ends. During his entire working life there was scarcely a month when some problem in this field was not engaging his attention.

Although Electric Discharge in Gases had all appearances to suggest that it might be a crucial and exciting part of the research front, it must be remembered that at the time it was a very limited field and almost entirely qualitative and descriptive, there were many spectacular effects that made popular demonstrations at lectures, but all efforts towards interpretation were vague and unsatisfying. THOMSON made a number of false starts in the subject, finding each time that the results though interesting fell below expectations and did not help much in finding any key to the relations between electricity and the structure of matter. Thus, he spent a great deal of time in measuring the speed of propagation of the luminous discharge in a vacuum tube many metres long — a formidable experimental problem with the Toepler vacuum pumps then available. He also made many observations of electrolysis of steam in a gaseous discharge, but found that the hypotheses on which he was working were untenable. Both researches, though long and difficult occupy no place in his later writings. Again, he continued for many years in an investigation of electric discharges in the absence of the disturbing effects caused by the presence of electrodes. He produced discharges in rarified bulbs by making the gas act as the secondary circuit of a high frequency transformer, the primary consisting of a few turns of wire around the outside of the bulb. The discharges obtained have since proved useful as spectroscopic sources, and also for studying afterglow effects, but at the time THOMSON must have been disappointed at the lack of any fundamental results.

At this point it is perhaps fitting to digress and examine THOMSON's character as an experimentalist and as a leader and teacher in the laboratory. As has already been stated, he was not particularly skilfull in working with his hands, and many people have described him as being even exceptionally clumsy. He seems however to have had a native genius for understanding the potentialities and limitations of all pieces of apparatus. He knew what devices to construct in order to make some desired observation; he knew how

to modify the apparatus to produce further results; he could diagnose any failings and errors and put his finger on the means of correcting such faults. Clearly it was most efficient for him to be the brain and for others to be his hands. His research students and laboratory assistants built up and tore down apparatus at his suggestion, and at time the pace must have been feverish and highly exciting. It is perhaps this haste in improvising and modifying apparatus that is responsible more than anything for the legend of «sealing wax and string» which grew up as a description of experimental methods of the Cavendish Laboratory. Some have interpreted this characteristic as a product of stinginess and lack of funds, but although funds were never particularly lavish, they were usually just sufficient for their needs, and the improvisation was probably not dictated by this but rather by the fertility and rapid succession of his ideas.

Since THOMSON was so much dependant on his assistants, it is also proper to record a tribute to his chief helper, EBENEZER EVERETT, who came to him as a young boy and stayed throughout his working life. Many of the important researches would have been impossible without the devotion and skill of EVERETT, and a share in the honours of THOMSON's great discoveries rightfully belongs to him. Just as he was dependant on his assistants, J. J. was also intimately bound up with the life of the laboratory as a whole, drawing on the skill and the ideas of his research associates there. He was the sort of man who absorbed ideas, soaking them up during conversations and later being quite unable to recall how the ideas had originated. Although he was always punctilious in giving credit to other workers, he may often have been unaware that part of his ideas had come from other people. This makes it even more difficult than usual to discuss the various priority claims that form a somewhat unsavory facet of the whole history of modern science. When an idea is in the air, it occurs to several people, and only rarely is an important discovery quite unique and unattended by very similar work by other hands. In the case of J. J., quite a large part of his major contributions may have been inspired by the work of his students, but this cannot lessen at all the genius of the leader of the laboratory in bringing together and recognising the importance of the field of research as a whole.

It is important to our estimation of the next few miraculous years that we examine closely the position in the laboratory by 1895. By this time, it must be admitted, THOMSON—or J. J. as he was universaly known by then—had become very much of a specialist in this one branch of physics. His was the first generation of men working in such an advanced state of science that narrow specialization was become sufficient and necessary for continued progress. The dangers of specialization need no elaboration today, but for J. J. and his workers in the Cavendish Laboratory it must be taken as a measure of their inspiration and faith in the subject and in their leader that so much of the

effort was turned in this single and perhaps rather disappointing subject. It is true that they were not committing themselves or their resources so heavily into the future as one needs to now, but nevertheless the little group of post-graduate mathematicians in the Cavendish had become mobilized around J. J. and his chosen field by 1895. They were ready to ride in on the great tidal waves that broke classical physics asunder and brought in a new era. But, almost as a prelude and preparation for the great discoveries of ROENTGEN, BECQUEREL and THOMSON himself, there were important changes in the Cambridge University regulations that meant much for life and work at the Cavendish Laboratory.

In April 1895 the University introduced new regulations that had the effect of allowing graduate members of other universities to come to Cambridge and to take a degree there. Prior to this the only people available for work in the laboratory had been Cambridge graduates who possessed College fellowships or had private means or who were able to hold one of the several demonstratorships (who were not paid more then £ 50–£ 100 p.a.) and other teaching posts necessary for the provision of instruction in elementary physics. The admission of post-graduate students from outside coincided with the provision of funds which enabled fine young research workers to take advantage of the opportunity; in 1896 the first awards were made of scholarships provided from the funds which had accumulated from the profits of the London Great Exhibition of 1851; these 1851 scholarships had first been awarded in 1891, but a change of regulations now provided that students should proceed to a university other than their own, and this brought many young scientists towards Cambridge. One of the first was ERNEST RUTHERFORD—later to succed J. J. as Cavendish Professor—and in the same year there was J. S. TOWNSEND and J. A. MCCLELLAND. The new arrivals brought with them fresh inspiration for the laboratory and for the Professor. THOMSON was most happy and most inspired as a leader of his research students, and now that the doors had opened wide, the Cavendish rapidly became a great center for physical research and teaching, not merely as the physical laboratory of Cambridge University but also as one of the most important institutions in the world scene. In spite of its own home-grown geniuses, the tradition of NEWTON had grown stale and was showing signs of being insufficient for the purposes of advancing science. The infusion of new blood under their young professor was exactly what was needed in Cambridge, and by a happy miracle, the school of physics was moving along just the right tracks to take full advantage of the new discoveries that were to come.

The lapse of 60 years has left very few people who can recall the tremendous excitement that attended ROENTGEN's announcement of X-rays, early in 1896. It was perhaps the first discovery in physics that created a public sensation and reached the headlines of newspapers. From the middle of January until

late in March of that year every paper and journal carried reports and conjectures, rumours and gross exagerations about the new rays. Every laboratory where glass could be blown and where physical apparatus was available was turned to studying the new phenomena and taking radiographs for their own and the public edification and for the medical men who were quick to seize the possibilities of photographing the living bone—indeed much damage was done, unknown to all, by the hour-long exposures then necessary to record the rays on a photographic plate.

Soon after New Year 1896, offprints sent out by ROENTGEN began to reach colleagues in England and a short and rather reserved comment appeared in *Nature* on 16 January. A week later, the wide interest had been awakened and a full translation of the original paper was published. By then too, X-rays had been demonstrated by J. J. at a meeting of the Cavendish Physical Society, and interestingly enough the first hand to be photographed (that of the wife of Dr. G. F. C. SEARLE) showed an unsuspected broken bone. But this was more for playing to the gallery, the real interest for THOMSON and others in the laboratory lay in the application of X-rays to their own special field. THOMSON reports it thus,

> « I had a copy of the apparatus made and set up at the Laboratory, and the first thing I did with it was to see what effect the passage of these rays through a gas would produce on its electrical properties ».

To his great surprise and delight he found that the effect was to make the gas a ready conductor of electricity, due of course to the ionization produced. This was of vital importance to his researches for he had long realized that the greatest difficulties in working with discharge in gases lay in the violence of the methods used to induce a discharge. The effect of electrodes he had been able to avoid by the use of induced electrodeless discharges, but before the coming of X-rays he had been unable to make electricity pass through a gas without either applying very great electric forces to break it down or by using hot gases and flames—all methods to which accurate means of measurements were not readily applicable, and the results obtained tended to be capricious. The new phenomenon not only provided J. J. and the rest of his workers with the fresh technique they needed, but at the same time the very existence of this phenomenon raised the very problem that was a key to later developments.

The position in the research front of physics just before the discovery of X-rays was curious and especially shaking for those who believed that science dealt with universal and certain truths. There were two camps of physicists with diametrically opposed views as to the nature of the cathode rays — one section, predominantly German physicists, who thought that they were electromagnetic waves of some sort, and another group, including most of the English

physicists, who thought they were negatively charged particles. Much could be said in support of either side, and it was very difficult to get a conclusive proof one way or the other. The greatest objection to the particle view was that the rays could apparently pass through a window of thin aluminium foil, but even here it was countered that the window might be absorbing and re-emitting radiation as a secondary cathode. Then again, if there were particles in discharges of this sort, it was reasonable to suppose that they had been formed in the gas by some process similar to dissociation in a liquid electrolyte. This hypothesis could be tested by examining the spectra given by discharge at either electrode. If dissociation occurred it would be reasonable to expect the negative and positive portions to become segregated and to show different properties. J. J. made several experiment of this sort but failed to detect any really significant difference; the carriers, if they were particles, did not seem to be formed by dissociation of the hydrogen molecule.

Experiments with X-rays indicated to THOMSON that they had the effect of producing carriers of electricity within the gas, and it now became possible to work with these carriers in the absence of cathode rays. J. J. found that the conductivity produced by the rays did not reach full value immediately the rays were switched on, nor did they stop immediately when the rays were switched off. Further, he found that the conductivity produced could be destroyed by passing the gas through a filter of glass wool or by applying an electric field across it; the first phenomenon pointed towards the agency of particles rather than waves, and the second showed that the particles were probably charged. He also had found that the current that could be passed through the conducting gas was only proportional to the applied potential difference up to a certain point, above this point the gas acted as if its conducting powers were saturated, thus suggesting again that X-rays could only produce a certain limited number of carriers in a given body of gas.

Returning to the study of Cathode Rays, THOMSON had already shown that PERRIN's experiment of collecting the rays in a Faraday cylinder and proving that they carried a negative charge could be extended. He modified the apparatus, first deflecting the rays by a magnetic field and showed that the negative charge still resulted — again an indication that the charge was being produced by particles affected by a magnetic field rather than by any sort of wave. He had also measured the velocity of propagation of some of the discharges and shown that they were only about 1/2000 that of light. There had been considerable difficulty in detecting and measuring the magnetic and electric deflection of cathode rays. The rays are notably deflected by even a weak permanent magnet, but HERTZ had completely failed to detect any influence by the rays on suspended magnetized needles, and this was regarded as one of the objections to the corpuscular theory of the rays. A much greater objection was the fact that no electrical deflection of the rays had ever been

detected. THOMSON set himself to investigate this phenomenon, and the research occupies a central position in the discovery of the electron.

His first attempt to deflect the rays electrostatically used, as others had done, a pair of parallel plates producing an electric field through which the rays were shot. It was a failure—producing no steady deflection when the current was running—but J. J. by a stroke of genius of experimental insight noticed that there was a slight flicker just as the deflecting current was being switched on and off. The historically-minded reader will notice an analogy with the effect, only noted as a transient phenomenon, when FARADAY attempted to detect electromagnetic induction. J. J. correctly interpreted the existence of this transient effect and the absence of a steady deflection. He saw that it was likely that the gas in the tube was setting up some sort of electrical barrier, preventing the field from deflecting the beam of cathode rays. To get rid of this impediment he set about the most difficult experimental task of producing a much better vacuum within the tube, removing the considerable quantities of gas condensed on the walls of the tube and on the electrodes. This was done by running the discharge continuously, day after day, and pumping all the time with the best vacuum apparatus then available — eventually he was successful and able to measure the electrostatic deflection and compare it with magnetic deflections.

The importance of these measurements is that they gave for the first time sufficient data to enable one to calculate the properties of the carriers of the waves. As early as 1884, SCHUSTER had been able to deduce from the magnetic deflection alone that if the particles had the charge and mass of hydrogen molecules, their velocity could only be of the order of 1/1000 of the expected magnitude. This was thought to be reasonable in view of the many collisions which these particles were thought to have with the molecules of the gas. THOMSON was now able to show that the electric deflection indicated the factor of 1/1000 should be associated with the charge and mass of the particles rather than with their velocity, and he was able to check this result by determining calorimetrically the energy carried by the rays. Further experiments with improved vacuum tubes, better-defined rays and with higher vacuum led him to more accurate quantitative results and he was able to put forward two very striking results of the greatest importance and significance. Firstly the ratio of the charge to mass (e/m) of the particles was of the order of 10^7 whereas the greatest value previously found was 10^4 for the atom of hydrogen in electrolysis; thus either the cathode ray corpuscles were much lighter than hydrogen atoms or they carried a much greater charge. Secondly, J. J. found that this ratio of e/m was the same whatever gas was used in the tube — the mean value for 26 different determinations was $2.3 \cdot 10^7$ and this was also shown to be independent of the material used for the electrodes and for the glass bulb of the cathode ray tube. This was such a spectacular result that he went

on to confirm the constancy of this value by measuring the ratio found for the carriers liberated by the action of ultra-violet radiation on metals, by hot wires, and by X-rays.

Not only had he found that the ratio of charge to mass was so much greater than for the smallest atoms, but he had also determined the velocity of the rays and found them enormously larger than normal molecular motions. The conclusions thus far were presented by him at a Friday Evening Discourse at the Royal Institution in London on 30 April 1897, and in the course of this he stated his opinion that his results « seem to favour the hypothesis that the carriers of the charges are smaller than hydrogen atoms ». At first his views did not make much impression on the scientific world, but although they rapidly gained momentum there could hardly be complete acceptance until separate measurements could be made to determine the charge and the mass of the particles. Even in the absence of these determinations though it was clear that a quite new type of particle, probably much smaller than the hydrogen atom, had been discovered. THOMSON called it a « corpuscule » and continued to use this term for many years, even after the name « electron » (first proposed by Dr. G. J. J. JOHNSTONE STONEY in 1891 as an « atom » of electric charge) had long been accepted by all other physicists. THOMSON himself had investigated mathematically the properties of such « atoms » of electricity long before his work on cathode rays, and he had reached the conclusion that such a particle, though it contained no ordinary matter would act as if it possessed a mass depending on its charge and radius. When the discovery of the electron was announced there were some who thought that the minute mass was probably all explicable as due to the charge, but THOMSON himself does not seem to have shared this opinion.

The next links in the chain of evidence were provided by two other workers in the Cavendish Laboratory, C. T. R. WILSON and J. S. E. TOWNSEND. WILSON had spent a few weeks in 1894 at the observatory which then existed at the top of Ben Nevis, highest of the Scottish hills. While there he had been much impressed by the beautiful optical phenomena shown by the clouds and mists, and on his return to Cambridge he set about some experiments on them using clouds formed by the expansion of moist air. The process of cloud formation turned out to be even more exciting than the optical effects, and he was led to consider the nuclei around which condensation took place after the grosser dust particles had been removed from the air. He rapidly realised that here there was a technique for studying individual molecules or atoms of some sort that were in an exceptional state which made them act as nuclei for the minute drops in the cloud, and he suspected that these particles might be charged atoms or ions. Just as this stage was reached the laboratory came alive with the work on X-rays, and like all other good specialists WILSON tried out X-rays on his own line of research. Again the miracle happened—it was

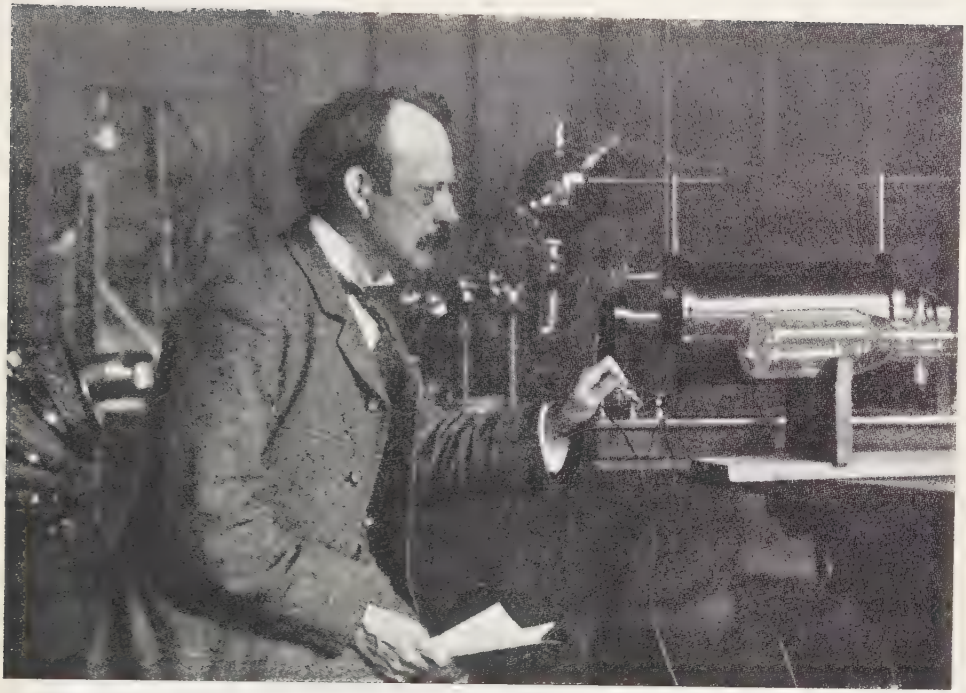

Fig. 2. Experiments on e/m, about 1902.

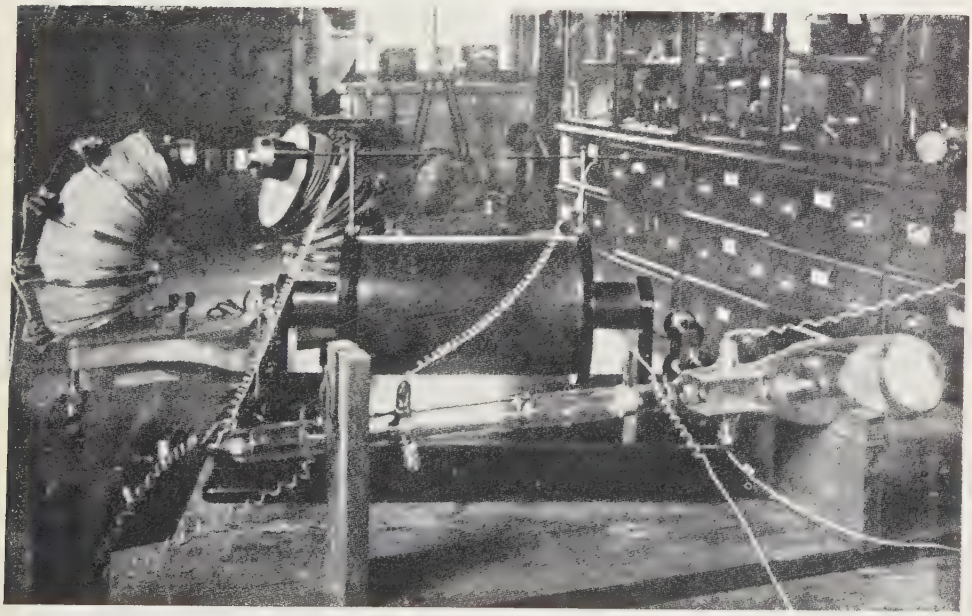

Fig. 3. – Another view of the Braun tube used for the later experiments on e/m.

exactly what was needed—and not only was there provided a clue which led THOMSON towards explaining ionisation by X-rays, but also WILSON was brought to the design of apparatus which is now familiar as his Cloud Chamber, an instrument more important than almost any other in the beginnings of atomic physics.

TOWNSEND, who with RUTHERFORD had been one of the first arrivals amongst the new research students, began an investigation of the subsidence of the clouds caused by condensation. He is responsible for the crucial suggestion that the rate of subsidence could be used to estimate the mass of the droplets, using a calculation of terminal velocity that had been made some time before by STOKES. With the mass of the droplets known it is a relatively simple matter to determine their charge by applying an electric field. TOWNSEND did this, finding a charge of 10^{-19} coulombs, but he failed to draw any far-reaching conclusions. THOMSON now took up the idea, and by measuring directly the current carried by a stream of ionized particles he was able to make a good estimate of the charge on an ion. Having by now measured e/m for particles in cathode rays and those produced by ultra-violet radiation and from an incandescent carbon filament, and also having determined the charge alone for ions produced by X-rays and by ultra-violet radiation, the case was becoming very strong. THOMSON communicated the whole research to the British Association, at their Dover meeting in 1899, under the title *On the existence of masses smaller than atoms* and it is perhaps from this point onwards that THOMSON's view received general recognition as an outstandingly important contribution to physics.

One may justly consider that the three years following ROENTGEN's discovery constituted a complete revolution in physics — the old, classical physics had cracked wide open. Not only was there the discovery of X-rays and all the work at the Cavendish Laboratory which has already been described, but also it was exactly at this time that HENRI BECQUEREL discovered radioactivity (1896) and his research was so greatly extended by the CURIES. Needless to say, the new phenomenon of radioactivity was treated in the Cavendish just as X-rays had been. Everyone was anxious to see what effect it would have on their current researches. For J. J. it was a new source of ionization, and he was able to demonstrate that once again the ionization was being caused by « corpuscles » smaller than atoms and having the same properties as before. The actual events of these few years were so breath-taking that they tend to conceal the fact that general changes in the laboratory and its status were remarkably great also. The end of the century may well be regarded as the close of an epoch. For the next few years THOMSON's main work was to consolidate the position by writing his book on the conduction of electricity in gases. It was published in 1903 and came out in a second edition in 1906, becoming a great classic of modern physical writing. A final

and expanded version was produced in two volumes (1928-33) by J. J. THOMSON and his son, G. P. (now Sir GEORGE) THOMSON who had just then demonstrated the wave properties exhibited in electron diffraction. It is perhaps one of the most remarkable personal occurrences in physics that a father and son should have worked independently and discovered the contradictory properties, particle and wave, of the electron.

Honours now began to shower on J. J. In 1905 he was appointed to a Professorship in the Royal Institution (thought still, of course, retaining his post in Cambridge), in 1906 he received the Nobel Prize, and in 1908 he was knighted. Later, in 1912, he was awarded the Order of Merit. Even Court precedent and etiquette could not turn him into « Sir JOSEPH », to all his colleagues and students he had been « J. J. », and now « Sir J. J. » he became. The next important series of researches carried out by THOMSON form a second peak in his career, starting slowly in 1905 and reaching a maximum around 1912, then being interrupted by the 1914-1918 war. The subject of these new investigations was complementary to his work on the electron; having reached an understanding of rays of negative electricity he now set about similar studies of rays of positively-charged particles.

GOLDSTEIN had found in 1886 that if holes or channels were made in the cathode of a discharge tube, rays streamed through making a luminous track and causing a phosphorescence on the glass bulb rather different from that produced by cathode rays. He called these new rays *canalstrahlen* and showed that they differed also from cathode rays in not being deflected by a magnetic field. In 1898, twelve years later, WIEN showed that they were deflected, but only by a powerful field and in a direction indicating that they were positively charged particles created from the residual gas in the tube. THOMSON found great technical difficulties in the way of experimental work on these positive rays. If there was insufficient gas in the tube no charged particles could be produced, on the other hand if the pressure was not very low, the discharge could not pass. He solved this difficulty in many stages; first by using a large bulb, then by making the channel very narrow—a hypodermic needle was used at one stage—and evacuating the bulb on one side. The key to successful working was eventually found to be the use of charcoal cooled by liquid air as a means of obtaining the high vacua required. Without such a technique much of THOMSON's later work would have been impossible. Fortunately the laboratory received a timely gift of a liquid air plant and the facilities it gave made a great deal of difference to all work in the Cavendish. By good fortune nearly all the models of the positive rays apparatus made by THOMSON have been preserved. One can readily see the changes made from the first few, quite unsuccessful patterns to the later finished articles which yielded much published data. The first model has a small bulb — far too small for its purpose. The second model rectifies this, the third adds the charcoal and

Fig. 4. – Measurement of positive-ray parabolas, about 1912.

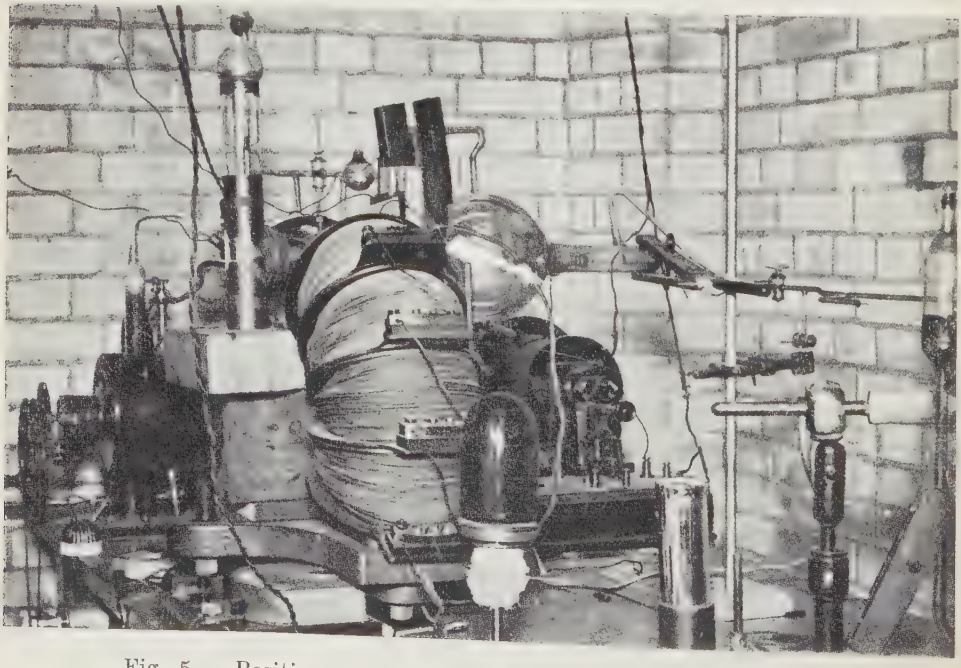

Fig. 5. – Positive-ray apparatus in the Cavendish Laboratory.

liquid air technique, and with this THOMSON got his first results. Deflecting the rays in one direction by an electric field and at right angles to this by a magnetic field, he found that the rays could be spread out into a series of parabolas, each one corresponding to a stream of charged particles of various velocities but with constant e/m. Assuming that the charges were units or simple multiples of the basic electronic charge (but positive), it was found that the masses corresponded to those of atoms and combinations of atoms in the tube. Subsequent photographic models enabled research to be continued with greater ease and accuracy of measurement.

The most important discovery was the detection of two separate parabolas in the case of the inert gas, Neon, samples of which had been obtained from DEWAR, THOMPSON's colleague at the Royal Institution. These parabolas indicated atomic weights of 20 and 22, showing that Neon existed normally as a mixture of these two isotopes — an occurrence which had been suggested previously by SODDY in the case of radioactive elements, but which had not hitherto been observed in stable substances. THOMSON was by no means certain of his findings: his apparatus was not sensitive to distinguish clearly between atomic weights of 20 and 20.2 (that of natural neon), and the value of 22 could have been due, J. J. thought, to the formation of a transient hydrogen compound, NeH_2. The later work by ASTON, who developed the technique further, producing the mass-spectroscope, confirmed also the existence of isotopes, not only in neon but in most other elements as well.

The first World War marks the end J. J. THOMSON's most active career as an experimentalist, though to the end of his days he continued to work and publish papers in physics. During the war the laboratory was turned over entirely to being a workshop for the manufacture of precision gauges, and J. J. became a member of the « Board of Invention and Research » which had been set up by the government to assist the ar effort. Neither the use of the laboratory nor of its professor would now be considered as wise, but in those days the military importance of physics was slight, and the Board proved to be an almost but not wuite useless waste of professional manpower. During this period J. J. served as President of the Royal Society from 1915-20; he had already served as Vice-President 1911-13. He was an efficient and popular President during those difficult years, smoothing over the cases of personal friction that were such a widespread occurrence in the rapid expansion of science then taking place, taking his place as an expert and consulted on general scientific matters by the government.

Early in February 1918 THOMSON was offered the Mastership of Trinity College, Cambridge. It is perhaps the most valuable of all permanent Cambridge appointments, rich in power, historical tradition and income, and the gift of it (unlike the masterships of other colleges) lies with the Crown, not with the College itself. The choice was a very natural one, for J. J. had been

associated with the college all his life and was the most distinguished of its members in residence, well-known and liked outside Cambridge, and of international fame as a scientist. At first J. J. thought that the Mastership would not hinder his work at the Laboratory, though he intended to give up the lectures and his professorial salary. With all the work of getting the laboratory running again it soon became evident that things could not proceed thus, but J. J. was still most anxious to keep contact with the Cavendish and to continue to work there so far as time allowed. The choice of THOMSON's most distinguished pupil, ERNEST RUTHERFORD, as successor to the Professorship was almost a forgone conclusion, but it required great tact and gentle negotiation to settle the question of the relative status and duties of the new Cavendish Professor and the special Honorary Professor, J. J. THOMSON. Finally, a room and his old assistants were placed at the disposal of J. J. and all the other research and teaching and the general management of the laboratory devolved on the younger man. Thus the greatest pages of the scientific life of J. J. THOMSON came to and end, but it was not by any means the end of the book, for he continued in his Mastership of Trinity and as an eminent public figure for more than two decades. His last contribution to the *Proceedings of the Physical Society* was a section of the obituary notice for Lord RUTHERFORD, whose life he had spanned.

In retrospect THOMSON's scientific work may be regarded as a singular point in the curve of development of science. This account has not striven to give details of all the other workers in the Cavendish Laboratory and elsewhere in the world who helped to bring about those changes in physics with which the name of THOMSON is to be associated. It would be invidious to claim that any one man is wholly responsible for even a single step in the advance, but to attempt to cite all the work, even in the Cavendish Laboratory alone, would be to write a complete history of modern physics. In historical perspective the importance of J. J. THOMSON is that he exhibited so clearly a whole series of contradictions which appeared in physics during his life-time. He was an individualist and his mind was «a province unto itself», yet he led a team and created a school around him. He was an experimental genius, yet his work was largely carried out by hands other than his own. Above all he had been educated well within the classical tradition of mathematical physics, and though he was a major participant in the breaking of its bounds he never took kindly to relativity or to quantum theory — accepting them but perhaps not feeling that they were part of the physics with which he was concerned. He opened the door into modern physics and standing on the threshold he held it wide for the 34 years for which he was Cavendish Professor.

HISTORICAL SOURCES

LORD RAYLEIGH: *The Life of Sir J. J. Thomson* (Cambridge, 1942).
J. J. THOMSON: *Recollections and Reflections* (London, 1936).
(Several authors): *A History of the Cavendish Laboratory* 1871-1910 (London, 1910).
ALEXANDER WOOD: *The Cavendish Laboratory* (Cambridge, 1946).
Obituary Notices of J. J. Thomson, in *Obituary Notices of the Royal Society*, and in *Proceedings of the Physical Society*.
D. J. PRICE: *The Cavendish Laboratory Archives*, in *Notes and Records of the Royal Society*, **10**, 139-147.

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SUPPLEMENTO AL VOLUME IV, SERIE X
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The Velocity of Light and the Evolution of Electrodynamics.

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1. – Introduction.

The rise of the velocity c from the humble status of a mere conversion factor for two systems of units to the dignity of a universal constant of nature is a most instructive story, illustrating as it does not only the main stages in the evolution of electrodynamics, but a variety of epistemological points in the development of scientific thought.

The first decisive step was the recognition of the identity of light vibrations and electromagnetic waves. Although this step is rightly associated in the annals of science with the name of MAXWELL, it is interesting to reflect that it was also taken, quite independently and from a different approach, by his Danish contemporary LORENZ. In some way, LORENZ' argument was more direct than MAXWELL'S: it stemmed in straight filiation from WEBER'S and KIRCHHOFF'S work; but it was of a more limited scope. Both LORENZ and MAXWELL had, of course, to break away from the conception of action at a distance; LORENZ did it by the formal introduction of retarded potentials, MAXWELL by a deeper analysis of the mechanism of propagation of electromagnetic action. LORENZ still proceeded in the spirit of the mathematical school of AMPÈRE, WEBER and KIRCHHOFF; MAXWELL took his inspiration

from the very different, more descriptive views of FARADAY. From this first momentous advance the constant c emerges with a clear, but still limited, physical meaning: it was the velocity of propagation of disturbances in a hypothetical material medium, the luminiferous aether, whose elastic properties had somehow to be described in terms of electromagnetic field intensities.

The next period, during which attention was centred on the properties of fast moving electrified bodies, culminated in a crisis which shook to its very foundations the science of mechanics, hitherto considered as the basis of all physical thinking. In the establishment of the theory of relativity, by which this crisis was solved, the velocity of light played an essential part: it was now regarded as a truly universal constant of nature, representing the greatest possible velocity of any physical process, be it the propagation of an electromagnetic wave or the motion of a material body. It is the latter conception of a maximum velocity of motion of all matter which is the great novelty of EINSTEIN's theory: it entails a radical change in the definition of the fundamental kinematic and dynamic concepts, beginning with the concept of simultaneity. But even though the relativistic account of electromagnetic phenomena does not differ in form from MAXWELL's theory, the whole position of electromagnetism in the general conception of the physical world has also undergone a complete transformation. The idea of the aether as a material medium has become inconsistent and the electromagnetic field must be treated as a primary constituent of nature on the same footing as material particles. By giving the principle of relativity its utmost extension EINSTEIN was further able to incorporate gravitation into the picture, and thus to round off the comprehensive description of physical phenomena which is now denoted as « classical ».

It is noteworthy that the conceptual frame of classical physics is essentially dualistic. In the course of time the dualism of matter and force underwent curious vicissitudes, but in spite of repeated efforts, it could never be reduced to unity. During the mechanistic period the aim was, naturally, to eliminate the concept of force. In HERTZ' mechanics, for instance, this is done by the expedient of « hidden masses », not more illuminating than DESCARTES' « matière subtile ». The failure to account for the electromagnetic phenomena in mechanical terms precipitated a dialectic reversal of the dilemma: the field concept was given the pre-eminence and the corpuscular appearances of matter were variously, and never convincingly, imagined as condensations or singularities in the field distribution.

The dilemma became quite acute after the discovery of the quantum of action (*) by PLANCK and the introduction of the concept of light quantum

(*) For the history of the first phase of development of quantum theory, see ref. [20], in which references to the original papers may be found.

by EINSTEIN. The question was no longer to decide in a general way whether particles could be explained in terms of fields, or vice-versa: in the case of electromagnetic radiation, one had to face the queer situation of the same physical agency behaving like a field or like a particle according to the conditions of observation. The very seriousness of the problem (which, at that early stage, was especially stressed by PLANCK) helped to bring it back from the somewhat hazy realm of inconclusive speculation to the foreground of theoretical and experimental investigation. The crisis experienced a further sharpening when NIELS BOHR, by linking the quantal properties of radiation with the problem of atomic structure, recognised in the existence of the quantum of action the keystone, not only of electrodynamics, but of the whole stability and constitution of matter. However, as BOHR aptly remarked [21], « it is just by bringing out the contrast that it may be possible in due course to give the new ideas a certain coherence ». In fact, the solution of the dilemma which eventually emerged from the further elaboration of quantum theory presents the character of the synthesis in which a dialectic process culminates: the dualism of particle and field is now effectively overcome by a synthetic conception in which they both occur as complementary aspects. The scope of this synthesis is universal: it has been found to apply not only to electromagnetic radiation, but just as well to old-fashioned « matter » and also, as the triumph of YUKAWA's prediction of the meson shows, to the new-fangled agencies responsible for nuclear interactions and transmutations. It achieves, in fact, a tremendous extension and unification of the concept of matter and puts the problem of the structure of the physical world in an entirely new light.

The relationship of complementarity between physical concepts completely elucidates the apparent paradoxes arising from an uncritical use of such concepts. In particular, it imposes reciprocal limitations upon their quantitative determination which render meaningless any attempt at a deterministic analysis of the classical type. In this connexion, the quantisation of the electromagnetic field raises interesting problems, into the solution of which the universal constant c enters in a new role, viz. is representing the dimension of velocity. For the consistency of the quantum theory of pure radiation fields, it is essential that this theory does not involve any scale of space-time dimensions, and therefore does not impose any restriction upon the construction of the idealised test-bodies serving to explore the field. Now, this property, which is reflected in the conformal invariance (or even the gauge-invariance in the sense of WEYL [22]) of the field equations and of the commutation relations (cf. ref. [23]), is an immediate consequence of the fact that the theory contains only two universal constants, c and h, which are clearly insufficient to fix units of space and time dimensions. In this case the particle, or photon, aspect does not introduce any non-vanishing mass constant, because the complementary field aspect deals with forces of infinite range.

After this preliminary survey of the winding path of history, it would be worth while dwelling a little longer at each of the three main turnings which opened up such unexpected vistas. In this essay, however, we shall not discuss the last two phases any further, but concentrate exclusively on the first. The conceptions and speculations which guided the pioneers have already receded into the haze of the past: it is by no means easy to think about these problems in terms now so unfamiliar to us: yet, the effort is amply repaid by the fascination of the great minds who struggled to solve them and the light their example throws upon essential features of the development of scientific ideas.

2. – From Weber to Lorenz.

The introduction of two distinct units of electric charge is a direct consequence of the conception of the current as a flow of electricity; for one has then the possibility of basing the measurement of charge either on the ponderomotoric action of charges at rest or on that of moving charges, expressed by AMPÈRE's law. The systematic representation of currents by the motion of charges was part of the atomistic theory of electricity initiated by FECHNER's somewhat erratic genius and brought to fruition by WEBER's unrelenting effort. In this theory, an electric current is imagined to result from the superposition of two flows of equal and opposite charge density with equal and opposite velocities. By means of this queer conception, WEBER [5 *a*] succeeded in reducing both the laws of electrodynamics and those of electromagnetic induction to a single formula giving the force between two elements of charge e, e' whose distance r varies with time in consequence of the motion of the charges. If the Coulomb force between the charges is added, the formula becomes

$$\frac{e\,e'}{r^2}\left[1-\frac{1}{c_{\mathrm{w}}^2}\left(\frac{\mathrm{d}r}{\mathrm{d}t}\right)^2+\frac{2}{c_{\mathrm{w}}^2}\,r\,\frac{\mathrm{d}^2 r}{\mathrm{d}t^2}\right], \tag{1}$$

WEBER's famous law of electrodynamic action. Since it embodies both electrostatic and electrodynamic forces, it contains a constant c_{w}, proportional to the conversion factor between the two units of charge.

Although the constant c_{w} was thus introduced by WEBER as early as 1846, its actual evaluation [5 *b*] was not carried out before the latter part of 1855. The main reason why this task was postponed to so late a stage in WEBER's systematic programme of basic measurements of electromagnetic quantities is the technical difficulty of the necessary experiments, especially the static charge determination, for which KOHLRAUSCH's expert assistance was enlisted.

WEBER had, of course, a rough idea of the expected order of magnitude of the unit ratio he had to measure, which has the dimension of a velocity; but its relationship with the velocity of light was by no means obvious. In fact, Weber's constant c_W, referring to electrodynamic rather than electromagnetic units, is not our c, but $\sqrt{2}$ times this quantity; therefore the numerical result he obtains, when he compares it with the value of the velocity of light, does not fire his imagination. He merely mentions the purely formal interpretation of the velocity c_W which is suggested by the electrodynamical formula (1) (or rather, as we know, by an illicit extrapolation of this formula), viz. the relative velocity which two charges must have so as not to exert any force upon each other; and he observes that the velocity of propagation of a disturbance in an elastic medium is something quite different, anyhow, from an actual velocity of motion of particles.

Nor is WEBER more impressed when he discovers, soon afterwards [5 *c*], that his law of electrodynamic action predicts the existence, in a *perfectly conducting* circuit, of periodic modes of oscillation of the electric current whose velocity of propagation has indeed the same value $c_W\sqrt{2}$ as the velocity of light. He cautiously points out that for a circuit of finite resistance these waves of current have neither a constant amplitude nor a unique velocity of propagation; in fact, for a very thin wire, the latter quantity has the form $c[1-(k\lambda/c)^2]^{\frac{1}{2}}$: it depends on the wave-length $2\pi\lambda$ and on the damping constant k, i.e. on the resistance and dimensions of the circuit. There is so little similarity between such waves and aether vibrations of the optical type that he not unreasonably doubts whether the near-numerical coincidence of their velocities of propagation has any significance: « it does not seem to be such », he declares, « as to raise great expectations » (« welche nicht derart zu sein scheint, daß sich große Erwartungen daran knüpfen lassen » [5 *d*]).

The derivation of the waves of electric current in conducting circuits had been carried out simultaneously by KIRCHHOFF [6 *a*], who forestalled WEBER in the publication of his less thorough, but essentially equivalent analysis. KIRCHHOFF lays somewhat more stress on the limiting case of perfect conductors and on the fact that the velocity of propagation in this case is independent of the state of the conductor; he also mentions its numerical equality with the velocity of light. However, he does not risk any comment on these points, and his lukewarm attitude later on towards MAXWELL's theory suggests that he must not have rated their significance much higher than WEBER did.

How refreshing, by contrast, is RIEMANN's youthful reaction! « My discovery of the connexion between electricity and light »: by these proud words he describes, in a letter to his sister [7 *a*], the contents of a short paper [7 *b*] which he submitted to the Göttingen Royal Society in February 1858. I understand, he goes on, that « GAUSS has set up another theory different from mine,

which he has communicated to his nearest friends. But I am quite convinced that mine is the right one and that in a year or two it will be generally acknowledged as such ». His idea was to derive the law of electrodynamic action from the conception of a finite velocity of propagation of this action, and to show that agreement with WEBER's results was obtained if this velocity was identified with that of light. With admirable directness he writes down the generalised Poisson equation, involving the operator now called « dalembertian », and the expression for its solution in the form of a retarded potential (*). He then founders, however, in the mistaken attempt to prove that the retarded *scalar* potential of the charge elements in two conductors is equivalent to the electrodynamic potential derived from WEBER's law. It is a strange freak of fate that RIEMANN, of all people, should stumble into one of those traps which Mathesis usually reserves for her less favoured servants: the unallowed permutation of two integrations! The discovery of this error, and the withdrawal of the paper, must have been a bitter disappointment for him; although he later repeatedly lectured on the theory of electricity and magnetism, he did not venture again into untrodden paths. RIEMANN's intervention thus remained an obscure incident (⁻) which did not influence the development of the subject; nevertheless it is well worth being put on record, as a fitting background against which to appreciate the extraordinary failure of KIRCHHOFF and LORENZ' splendid achievement.

Shortly after his work on the movement of electricity in wires, KIRCHHOFF published, in the same year 1857, another paper [6 *b*] in which the fundamental equations were written down in the local form appropriate to a continuous conducting medium. To this end, he now uses the concept of current density $\boldsymbol{j}$ connected with the density ϱ of free electricity by the continuity equation ( )

$$\operatorname{div} \boldsymbol{j} + \frac{\partial \varrho}{\partial t} = 0 \,. \tag{2}$$

OHM's law is assumed to hold locally even for a non-uniform distribution of electricity. Calling σ the coefficient of conductivity, it may be written

$$\boldsymbol{j} = \sigma(\boldsymbol{E}_{\text{el}} + \boldsymbol{E}_{\text{ind}}) \,, \tag{3}$$

(*) The first *published* occurrence of retarded potentials is in a paper of 1861 on the theory of elasticity by L. LORENZ [8 *b*].

(⁻) The withdrawn paper was published posthumously in 1876, and immediately criticised by CLAUSIUS.

(⁻) I slightly modernise the argument by using the vector notation and the electromagnetic units rather than the electrodynamic ones, i.e. directly the factor c rather than c_{W}. The current density is expressed in electrostatic units.

where $\boldsymbol{E}_{\text{ind}}$ represents the induced electromotive force, derived from WEBER's law, and $\boldsymbol{E}_{\text{el}}$ the e.m.f. arising from the distribution of free electricity, for which KIRCHHOFF upholds the expression valid for a static distribution:

$$\boldsymbol{E}_{\text{el}} = -\operatorname{grad} V\,, \qquad \text{with} \qquad V(\mathrm{P}) = \int \frac{\varrho(\mathrm{P}')\,\mathrm{d}v_{\mathrm{P}'}}{r_{\mathrm{PP}'}} \tag{4}$$

($r_{\mathrm{PP}'}$ denotes the distance of the points P, P′ of the medium). For the induced e.m.f. one may write (*)

$$\boldsymbol{E}_{\text{ind}} = -\frac{1}{c^2}\frac{\partial \boldsymbol{A}}{\partial t}\,, \quad \text{with} \quad \boldsymbol{A}(\mathrm{P}) = \int \frac{\boldsymbol{j}(\mathrm{P}')\,\mathrm{d}v_{\mathrm{P}'}}{r_{\mathrm{PP}'}}\,. \tag{5}$$

The potentials V, $\boldsymbol{A}$ satisfy the Poisson equations

$$\Delta V = -4\pi\varrho\,, \qquad \Delta \boldsymbol{A} = -4\pi \boldsymbol{j} \tag{6}$$

and the auxiliary condition

$$\operatorname{div} \boldsymbol{A} + \frac{\partial V}{\partial t} = 0\,. \tag{7}$$

From this set of local equations KIRCHHOFF derives again those he used in his first paper for thin conducting wires. If i denotes the current intensity and e the charge contained in an element of unit length of the wire, the scalar and vector potentials at a point of the wire (the latter projected along the tangent to the wire) are readily seen to be approximately proportional to e and i respectively, with the same factor of proportionality γ, which depends only on the dimensions of the circuit. Thus, eq. (3), with (4) and (5), leads to a relation of the form

$$i = -R^{-1}\gamma\left(\frac{\partial e}{\partial s} + \frac{1}{c^2}\frac{\partial i}{\partial t}\right). \tag{8}$$

(s is the length of arc of the wire, R its resistance per unit length), while

(*) This is actually the formula proposed by F. NEUMANN in 1845 (see ref. [19], chapt. VII). WEBER's law leads for the vector potential to the equivalent expression

$$\int (\boldsymbol{j}(\mathrm{P}')\cdot \boldsymbol{r}_0)\,\boldsymbol{r}_0\,\mathrm{d}v/r_{\mathrm{PP}'}\,.$$

($\boldsymbol{r}_0$ is the unit vector along PP′), which is also adopted by KIRCHHOFF.

eq. (2) yields

$$\frac{\partial e}{\partial t} + \frac{\partial i}{\partial s} = 0 \,. \tag{9}$$

Elimination of e leads to the wave equation

$$\frac{\partial^2 i}{\partial s^2} - \frac{1}{c^2}\frac{\partial^2 i}{\partial t^2} - \frac{R}{\gamma}\frac{\partial i}{\partial t} = 0 \,, \tag{10}$$

on which KIRCHHOFF's and WEBER's previous discussion of current waves was based. Now, however, KIRCHHOFF was more anxious to treat the opposite extreme case of long circuits, in which no periodic currents are possible; this problem, first tackled two years before by W. THOMSON, was then of immediate practical interest in connexion with the laying of the first transatlantic telegraph cable.

While the integrated equation (10) has a wide domain of approximate validity, KIRCHHOFF's local equations are incomplete and lead to wrong consequences; for the distribution of electricity, e.g., one finds that the expression $(\partial\varrho/\partial t) + 4\pi\sigma\varrho$ would be $= -(\sigma/c^2)(\partial^2 V/\partial t^2)$ instead of $= 0$. The effect of the displacement current, which is, of course, the lacking element, could be introduced—as MAXWELL [15] does—simply by defining the vector potential in terms of the *total* current rather than the conduction current; more elegantly, however, by taking for *both* V and $\boldsymbol{A}$ retarded potentials (i.e. replacing the Poisson equations (6) by the corresponding « dalembertian » equations). This is just the decisive step which was taken by LORENZ [8 *a*], when in 1866, he takes up the thread of the argument left by KIRCHHOFF in such an inconclusive state.

Inspired by the great idea of the unity of the forces of nature, LORENZ was consciously looking for an analogy between light and electricity. Just like RIEMANN, he realises that the first feature which such an analogy must embody is the finite propagation of electrodynamic actions; and he knows from previous investigations [8 *b*] how to express this property by means of retarded potentials. He has the enormous advantage over RIEMANN, however, of being able to start from the much better basis afforded by KIRCHHOFF's paper. The local equations established there, representing electrodynamic phenomena in an indefinite medium, were indeed, in contrast to the law of interaction of elementary charges considered by RIEMANN, directly suited to a comparison with the phenomenon of light propagation. The introduction of retarded potentials into KIRCHHOFF's equations does not appreciably modify their consequences within the domain of quasi-stationary electrodynamics, which, as LORENZ pointedly emphasises, is their only empirical basis. The local distribution of charge and current, however, as well as their variation

in time, are radically altered. From eq. (3), taking eq. (4) into account, one derives successively

$$\frac{\partial \boldsymbol{j}}{\partial t} = \sigma \operatorname{grad} \operatorname{div} \boldsymbol{A} - \frac{\sigma}{c^2} \frac{\partial^2 \boldsymbol{A}}{\partial t^2} ,$$

$$\operatorname{rot} \operatorname{rot} \boldsymbol{j} = \frac{\sigma}{c^2} \operatorname{rot} \operatorname{rot} \frac{\partial \boldsymbol{A}}{\partial t} ,$$

whence

$$- \operatorname{rot} \operatorname{rot} \boldsymbol{j} - \frac{1}{c^2} \frac{\partial^2 \boldsymbol{j}}{\partial t^2} = - \frac{\sigma}{c^2} \frac{\partial}{\partial t} \left(\Delta \boldsymbol{A} - \frac{1}{c^2} \frac{\partial^2 \boldsymbol{A}}{\partial t^2} \right) .$$

If, therefore, the velocity of propagation entering into the dalembertian equation for the vector potential is identified with the constant c of electrodynamics, the last equation takes the simple form

$$- \operatorname{rot} \operatorname{rot} \boldsymbol{j} - \frac{1}{c^2} \frac{\partial^2 \boldsymbol{j}}{\partial t^2} = \frac{4\pi\sigma}{c^2} \frac{\partial \boldsymbol{j}}{\partial t} . \tag{11}$$

Now, LORENZ recognises in the left-hand side of this equation the expression which, equated to zero, represents the propagation *in vacuo* of the light vector in the elastic theory of light which he had developed some time before. The essential point, which he immediately stresses, is that eq. (11) describes current waves which are purely transversal: this clinches the argument for the identification. A further confirmation lies in the relation, exhibited by eq. (11), between absorptive power and conductivity; a perfectly transparent medium, in which the velocity of propagation has the value c, is one of *vanishing* conductivity.

LORENZ is quite clear about the far-reaching consequences of his work. Above all, he perfectly realises that the local equations are the fundamental expression for the laws of electromagnetic interactions, and that therefore the whole conception of action at a distance has to be abandoned in favour of that of interactions between neighbouring elements, advocated by FARADAY. More remarkable still is his concluding remark about the further problem of reducing the electrical currents to some sort of motion of particles, which is of course the ideal aim of mechanistic philosophy: after some suggestions of striking originality, he emphasises that in any case the main result of his investigation, the identity of light vibrations and current waves, « does not rest, and therefore does not depend either, upon any physical hypothesis ». Nothing could better illustrate how far the dialectical process which was going to transform the common view of the physical world by the end of the century had already accomplished itself in the mind of the lonely Danish thinker.

3. – The philosophical background.

> La science découvrira (ce que beaucoup de philosophes ont vu) qu'ici encore, la vérité est dans l'action, dans le mouvement de la pensée, et non pas dans telle ou telle position déterminée.
>
> H. HOFFDING [24]

3·1. *Mechanicism and phenomenology.* – It is indeed time to pause at this stage of the story and try to appraise its significance for the evolution of physical thinking as a whole. The three protagonists, WEBER, KIRCHHOFF and LORENZ, are representatives of the three main currents of ideas which, by the middle of the XIXth century, were putting up rival claims to leadership in science and philosophy. The overruling influence was still that of the mechanistic ideal originating in NEWTON's revolutionary conceptions and elaborated by LAPLACE to an all-embracing system of natural philosophy. A first crack in this proud edifice had been the failure of the corpuscular theory of light; a less apparent, but in fact more serious undermining of its foundations resulted from its inability to respond with sufficient flexibility to the question of the mutual relationships between the various natural agencies, to which the requirements of a new phase of economic development had given an overwhelming prominence. As a result, two tendencies had arisen among the new generation of physicists: one of retreat, one of challenge; both threatening the positions of mechanistic philosophy.

The programme of reducing the world to matter and motion was common to all the pioneers of science in the XVIIth century: it stemmed from a natural urge to explain the phenomena in terms of familiar experience. This programme only took shape, however, after the prolonged struggle between the adepts of DESCARTES and those of NEWTON had ended by the latter's victory; this was not before the middle of the XVIIIth century. There then emerged a somewhat streamlined newtonianism, from which all the deeply significant hesitations of the creator's thought, arising from his intimate insight into the difficulties of the subject, had been ironed out by less subtle epigons. This system (in contrast to the cartesian one) was essentially atomistic, its specific feature being the conception that the atoms are centres of forces acting at a distance, on the model of gravitation. LAPLACE gave this « physique des forces centrales » its consecration, not so much by formulating its principles in a sharp and lucid way, and showing the universality of its scope, as rather by demonstrating the real power of the seemingly vague and hazardous approach to the analysis of the properties of matter which the atomic hypothesis offered. In his theory of capillarity he showed how the phenomena could be derived in every detail from the mere assumption that the molecular interactions are of short range [1]. It was permissible, therefore, to treat the atoms as structureless sources of

central forces; in fact, speculations about their structure are deprecated as »vagues et inutiles à l'avancement des sciences» [1]. The true significance of the atomic theory (at this early stage) is quite rigthly seen by LAPLACE in its suggesting relationships between phenomena which would otherwise appear disconnected; thus (he adds with characteristic emphasis on the pratical role of science) giving rise to chemical theories with increased possibilities of «applications aux arts» [1].

The ban on arbitrary speculations about the structure of atoms, thus imposed by LAPLACE, was undoubtedly a source of strength for mechanistic physics; but it also contained a seed of discord, which, as soon as partial failure bred disillusion, led to a weakening of the mechanistic positions. It invited cautious minds to distrust atomistic considerations to such an extent that they regarded it as desirable or even imperative to eliminate them altogether from the formulations of physical laws: the aim of physical theories was simply to give an accurate «description» of the phenomena in terms of concepts which could be unambiguously referred to observation. This «phenomenological» (*) school, of which KIRCHHOFF was one of the most authoritative exponents, did not explicitly reject the tenets of mechanistic philosophy, but was content to leave the attainment of its ideals to a dim and distant future. it did not deny the existence of the atoms, but pretended to ignore them;

Originally, this attitude arose as a simple matter of practical policy in scientific method; in this sense it is lucidly set out by AMPÈRE [4*a*]: first ascertain the laws which give an accurate account of the observations; then proceed to seek their relation to the assumed structure of the physical world. So long as the criticism of mechanistic speculations remained at this methodological level, reflecting real uncertainties in our knowledge, it could only be stimulating. Inherent in the method itself, however, was the danger of emphasising formal relationships at the cost of concrete physical reality: the unity which it was the role of the atomistic picture to provide came to be sought in mere formal consistency. This danger was very soon illustrated by the vulgarised version of phenomenology given in COMTE's system of «philosophie positive»: by representing the qualitative differences between the manifestations of the various natural forces as irreducible, just when the question of their mutual transformations and deeper-lying connexions was entering a critical stage, it had, in the French school, a stifling influence upon the progress of this fundamental inquiry.

3·2. *Weber and Kirchhoff.* – The impact of the two currents of ideas so far considered upon the development of electrodynamics is very conspicuous.

(*) This apt, but ugly denomination was only introduced towards the end of the XIXth century; I apologise for the anachronism and the cacophony.

Right from the beginning, through AMPÈRE's dominating intervention, the fundamental laws of interaction of currents and magnets were cast in the mould of central force physics. WEBER went on in the same spirit; in fact, he set himself the task of developing the mechanistic conception of electricity and magnetism to its utmost consequences. In particular, he endowed the carriers of electricity with inertia, thus originating the concept of *electron*. Although in a sense premature, since it was as yet incapable of experimental test, this idea gave WEBER unfailing guidance in the elaboration of his theories; in fact, as we know, only the electron concept could lead to an essentially correct conception of such phenomena as diamagnetism.

A good example of WEBER's method, which at the same time illustrates its difference from KIRCHHOFF's, is afforded by the question of finding the relation between current intensity and e.m.f. in the case of variable currents (which, as we saw, played an important part in the theory of current oscillations). KIRCHHOFF takes the purely formal course of extending OHM's law to this case, and hoping for the best; WEBER, consistent with his mechanistic picture, starts from the equations of motion of the electrons and appeals to OHM's law only to fix the value of the force exerted by the « ponderable matter » on the electrons in terms of the empirical conductivity coefficient. He accordingly obtains an equation differing from KIRCHHOFF's by an inertial term involving the time-derivative of the current intensity and depending on the unknown charge-to-mass ratio γ of the electrons. The effect of this term is merely to multiply the coefficient of $\partial^2 i/\partial t^2$ in eq. (10) by the constant factor $[1 + (c^2/\eta\gamma e)]$. Thus, WEBER's treatment does not modify the main argument; but, as WEBER points out in some detail, it opens the way, at least in principle, to a determination of the product ηe (involving the charge, mass and number per unit volume of the carriers of electrical current), i.e. to the investigation of a quite new aspect of electrical phenomena.

3·3. *Dynamical philosophy.* – However, in spite of its suggestiveness and its powerful formal resources, central force physics was doomed to ultimate failure in the domain of electromagnetic phenomena, where the basic idea of instantaneous action at a distance meets with an essential limitation of its validity. To break the spell of action at a distance required fresh inspiration. It came from a third current of ideas, to which we must now turn our attention. Phenomenology was not the only reaction to the shortcomings of the mechanistic views in the first half of the century. Especially those physicists who gave prominence to the idea of force, urged upon them by the practical problems raised by the growing utilisation of new sources of power, found little help in the current ideas, and many came to reject atomism altogether. We may conveniently call them « dynamical » physicists, although there was among them no doctrinal unity at all. The two protagonists of dynamical philosophy

with whom we are here concerned are ØRSTED and FARADAY; another great pioneer of the same tendency is ROBERT MAYER. These three thinkers developed their ideas in quite different environments without any mutual influence (except for the trivial fact that FARADAY built upon the foundation laid by ØRSTED's discovery), and apart from a general similarity of outlook, their views took rather different and highly individual shapes. We will now analyse ØRSTED's ideas and trace their influence on LORENZ.

3·4. *Ørsted.* – ØRSTED was consciously metaphysical in his approach to physics; he upheld more explicitly and consistently than most scientists of his time the XVIIIth century belief, equally shared by the French mechanicists like D'ALEMBERT and their German opponents of the Leibnizian school, that science could be established upon *a priori* principles regarded as necessary elements of our thinking. As a young man he eagerly followed the trend of philosophical speculation initiated by KANT in Germany, even when it dissolved itself into the mists of SCHELLING's *Naturphilosophie*. He stopped short, however, of the last, synthetic, phase of this evolution, when the subsiding mists brought to view the weird shape of HEGEL's system. By that time, ØRSTED's sound physical intuition had got the better of his romantic propensities, and he settled down to a view of the physical universe which embodied in sober scientific form valuable conceptions due on the one hand to KANT's emphasis of the dynamical aspect and on the other to SCHELLING's deep sense of the unity of nature (*).

ØRSTED adopted, with some variations, the Kantian view that the essential

(*) It is significant that, although his general philosophy of life was a form of spiritualism, ØRSTED rejected HEGEL's system because of its idealistic cosmology, whose topsy-turviness he exposes, in a most revealing short note [3*b*], with a pungency which would have done honour to MARX. His main point is that the evolution of the universe had been going on for a long time before reasonable beings appeared on the scene; the universe is therefore not produced by thought, but « thought is nature in us, awakening to our consciousness ». He is so clear about the implications of his criticism that he takes the trouble to warn us that these « indubitable truths do not throw us into materialism » because « the essence of all nature ... is spiritual »!

This is only one of many examples of the cleavage which took place in the XIXth century in the minds of most scientists between their class-determined spiritualistic beliefs and their scientific philosophy, which could not help being materialistic. For this reason I have deliberately refrained from referring the various tendencies discussed in the text either to materialism or to idealism: in my view, such labelling would be futile and even misleading; the ideological conflict symbolised by these two words was then fought in the social sphere, not in that of science. Marxist doctrinaires may remind me that one should not separate science from its social background: but what can I do if the scientists themselves chose to make this very separation in their own minds? *Why* they behaved like this is of course a most interesting sociological problem, which I have, unfortunately, no space to tackle here.

element underlying the phenomena which we describe as « matter » is some distribution of fundamental forces in space. The necessity of providing an explanation for permanence and stability as well as for change and motion naturally led, in this view, to imagine in every aspect of nature a « conflict » of opposing forces, resulting in rest or motion according as attraction and repulsion did or did not balance each other. This « dynamical » picture of the phenomena harmonised with SCHELLING's conception of the universe as a process of continual change, in which qualitatively different aspects appeared as various manifestations of the interplay of the same fundamental forces: hence the conclusion that all the agencies of nature are interrelated and can be transformed into each other. Thus, ØRSTED pictures the electric current as the result of a conflict of electric forces, whose balance was upset and restored in continuous succession; and this representation led him to suspect that the repercussions of the electric conflict in the space around the conductor might give rise not only to heat and light phenomena, but also to those effects which we describe as magnetic. Experiment, confirming this last suspicion, at once made the conception of the electrical conflict more precise, by suggesting that this conflict must be accompanied by some kind of rotational motion around the axis of the current [3 *a*].

Tenuous as such ideas may appear, it is a remarkable fact that their suggestive power was strong enough to put men like ØRSTED and ROBERT MAYER on the track of great discoveries: of course, they could not, and did not, provide more than an inspiration, inclining the mind in the right direction; the actual discovery could only be the result of the usual kind of rigorous logical thinking about skilfully selected observations. This essential identity of method, however, obscured by the unfamiliar twist given to familiar concepts, was not properly recognised by the contemporary mechanistic school: the perfect rigour of MAYER's argument was not appreciated (not even by his generous champion TYNDALL), and ØRSTED's careful electrodynamical experiments remained almost completely ignored. To be accepted, the new principles had first to be formulated into the current language: when the French physicists, for instance, received the announcement of ØRSTED's discovery, they did not stop a moment to consider what he meant by his « electrical conflict »; with utter contempt for an idea so much lacking in cartesian clarity, AMPÈRE attacked the problem afresh with the refined methods of central force physics. Thus, the reaction of the mechanicists to the dynamical opposition was simply a passive one: lack of understanding for its aims and methods, lack of interest for the vast, if somewhat hazy, prospects it opened. In fact, the mechanistic philosophy, as early as 1820, was so rapidly losing its flexibility that it was becoming unaware of the hypothetical character of its own postulates: thus, no less a thinker than AMPÈRE could describe his formulation of electrodynamics as « uniquement déduite de l'expérience » [4 *a*]. Above all,

the « dialectical » way of looking at things, which with ØRSTED makes its first entry into physics, was quite foreign to mechanistic thinking; indeed, it has remained so until our own days, when it has been finally forced upon physicists by the advent of quantum theory.

3·5. *Lorenz.* – ØRSTED's influence on scientific thought was naturally stronger in his own country, where he enjoyed immense prestige. There is no evidence that he had any personal contact with LORENZ (he died in 1851, when LORENZ was a student at the Technical High School at Copenhagen); but we find distinct echoes of his teachings in the latter's original and vigorously expressed ideas. In the first place, LORENZ, like ØRSTED, acknowledged as his guiding principle the idea of the unity of natural forces. In the second, he shares ØRSTED's dislike of mechanistic hypotheses, but not for metaphysical reasons: it is to the arbitrariness of these pictures that he rightly objects. Accordingly, he regards as the foremost task of physics the precise formulation of the laws of the phenomena in terms independent of any such hypotheses. In this, he does not differ from the phenomenologists, but his belief in an underlying unity prevents him from stopping at the formal, descriptive stage, and urges him onwards in a search for analogies which might reveal an essential identity of natural agencies whose manifestations appear qualitatively different. He has the great advantage over ØRSTED (like MAXWELL over FARADAY) of being a skilled mathematician, and he thus avoids the vagueness which so much weakened the case of dynamic philosophy.

LORENZ' great paper on the identification of light with oscillating electrical currents clearly illustrates these points. This paper is the culmination of a persevering inquiry into the nature of light, which he started in 1860; he soon abandoned the beaten track as he recognised the lack of cogency of any mechanical interpretation in terms of some aether model; he deliberately aimed at a purely abstract description of the laws of light propagation, and he succeeded in showing that under very general conditions these laws could be concisely expressed by a wave equation for transversal vibrations

$$\mathrm{rot}\ \mathrm{rot}\ \boldsymbol{u} + \frac{1}{a^2}\frac{\partial^2 \boldsymbol{u}}{\partial t^2} = 0\ .$$

Moreover, he found how to connect the light vibrations with their sources by means of retarded potentials. He was thus well prepared, when he saw KIRCHHOFF's paper, to take up the clue and make the decisive step. The essential point, however, is that, in contrast to WEBER and KIRCHHOFF, he was all the time looking for some analogy between light and other phenomena.

The case we are discussing shows the atomistic point of view (here represented by WEBER) decidedly at a disadvantage in comparison with LORENZ' « dynamical » position. In order to get a balanced assessment of the respective

merits of the two opposing conceptions, however, we must not lose sight of the fact that in the other great creation of XIXth century physics, the mechanical theory of heat, they drew level: while ROBERT MAYER was of the « dynamical » type, definitely hostile to atomism, his rival JOULE found a sure guidance in the kinetic conception of heat. Soon afterwards (1857-60), CLAUSIUS' and MAXWELL's brilliant elaboration of the kinetic theory of gases heralded a momentous advance in the evolution of scientific ideas; for this theory provided the starting point for the first fruitful synthesis of the dynamical and atomistic viewpoints: statistical thermodynamics, a synthesis which will doubtless remain for ever one of the main pillars of the temple of Nature.

It is interesting to watch LORENZ' changing attitude to this problem: about 1850, in his earliest (never published) investigations [8 *c*], he attempted to establish a relation between heat and light, on the basis of a complicated aether model: but he was quick, following the lead of the pioneers, to realise the heuristic value of the kinetic theory, and he adopted it in his later work, albeit with a keen eye for its difficulties and limitations. To appreciate LORENZ' foresight and shrewd judgment in this matter, one has to reflect that he, who had so forcefully emphasised the ambiguities of mechanistic ideas, made this choice at a time when the new advance of atomistic theory was challenged by a sharpening of the phenomenological criticism—encouraged, it must be said, more by a reaction accompanying the economic and social evolution (*) than by any inherent necessity in the development of physics.

While the idea of the unity of force, which had by then found its quantitative expression in the law of conservation of energy, was travestied in the ill-founded phenomenological doctrine of « energetics », it continued to inspire in a most fruitful way LORENZ' profound investigations. After his recognition of light as an electromagnetic phenomenon, he engaged upon a protracted study of optical refraction and dispersion (1869-83), which he undertook with the view of elucidating the molecular constitution of material media from the way in which the molecules of matter interact with light. Concurrently, he discussed the relationships between heat and electricity (1870-72), again on an atomistic basis, and derived (by arguments lacking in rigour, it is true) such striking results as the temperature-dependence of the WIEDEMANN-FRANZ constant and the number of molecules in an equivalent-gram of an electrolyte.

Thus, the identification of light vibrations with electric currents is by no means an isolated incident, a stroke of luck in LORENZ' career: it is the first, and most successful, stage in a quest of very wide scope pursued with remarkable singleness of purpose. The conclusions of LORENZ' paper are brief, but

(*) The transition of capitalism to the imperialist stage brought with it that predominance of idealistic and spiritualistic conceptions which is still paralysing the philosophical thinking of the bourgeoisie to-day.

they go to the root of the matter. Pointing out that the differential equation (10) allows of a simpler extension to heterogeneous media than eq. (3), he regards it as the more fundamental one, and stresses the fact that it corresponds to a conception of transmission of electrical action from element to element of the medium, in conformity with FARADAY's views. He also mentions AMPÈRE as having suggested such a view; this refers to an interesting discussion by AMPÈRE [4 *b*] of a possible mechanism of transmission of the attraction between two conductors carrying currents by a « neutral fluid » surrounding the conductors. AMPÈRE, like all clear-sighted adherents of the concept of central force,—in fact, like NEWTON himself,—was perfectly aware of the provisional character of this concept and of the desirability of underpinning it by some transmission mechanism; but he rightly thought that this last problem would not for a long time be capable of unambiguous solution.

The detailed picture of the transmission of electrical action outlined by LORENZ retains the rotational character which ORSTED had already recognised as a feature of his electrical conflict; it is so striking, in the light of our present knowledge, that it deserves to be quoted *verbatim*: « If we imagine light as a *rotational* vibration in the interior of the bodies around axes whose direction is that which in the elastic theory is regarded as the direction of vibration, the electric current will not be any translatory motion, but just a continued rotation in a definite direction, and the direction of the rotation axis is then the direction of the current. This rotation can only persist in good conductors, and the motion is then propagated along the direction of the axis; while in bad conductors it becomes periodic and is propagated, by what, in the theory of electricity, is called induction, in a direction perpendicular to the axis of rotation. On this view, there is hardly any reason to uphold the hypothesis of an aether, since one may very well assume that in all the so-called empty space there is enough material substance left to form a sufficient substratum for the motion. »

This may perhaps appear a mere adumbration of MAXWELL's powerful vision; at any rate the sketch is aptly drawn, and shows all the essential features of the accomplished masterpiece. The last sentence of the passage quoted betrays the particular aversion to the aether concept which LORENZ had formed in the course of his study of the mechanical theory of light, when he saw how little helpful and unreliable it was. The immediately following paragraph, on which the paper ends, is more clearly revealing of his detached attitude with regard to all mechanistic constructions: it is the remark, already alluded to above, that his main conclusion is in any case independent of any « physical », i.e. mechanical, hypothesis. Here LORENZ' thought is decidedly more advanced, thanks to its non-mechanistic component, than MAXWELL's: the latter will only reach a similar point of view, as we shall see, at a late stage in the elaboration of his theory. Taking leave with due respect of a work which

so worthily crowns the endeavour of the Continental school of electrodynamics, let us now cross the Channel and watch the developments which brought electromagnetic theory to full completion.

4. – Faraday and Thomson.

4·1. *The British school.* – MAXWELL's twin sources of inspiration, like those of LORENZ, were on the one hand a powerful dynamical conception of nature, that of FARADAY; on the other the great mathematical tradition of the French school. But the emphasis in this case was much more on the dynamical side: the French ideas came to him through the channel of WILLIAM THOMSON, who (himself under FARADAY's influence) had been more struck by FOURIER's mathematical treatment of the process of heat conduction as one of continuous transmission than by the atomistic trend of the Laplacian methods. Yet neither THOMSON nor FARADAY stood in any radical opposition to the atom concept: but they insisted on the primary importance of the dynamical aspect of the phenomena and tended to regard atoms as singular occurrences in an essentially continuous medium transmitting force through space. FARADAY was content to think of the atoms as structureless centres of force; THOMSON wanted to visualize them as definite structures arising from the continuous aether by some mechanical process: thus, as soon as he heard of HELMHOLTZ' beautiful theory of vortices (1867), he started imagining the atoms as vortices in the aether and developed this picture in great detail, and with evident relish. Anyhow, the mainspring of the phenomena was for both of them some distribution of mechanical stresses in the continuous aether.

This blending of atomistic and dynamical conceptions cannot be described as a synthesis in the dialectical sense. It certainly paved the way for a true synthesis in the dynamical theory of heat, as MAXWELL's later contribution to the foundation of statistical mechanics clearly shows; but in the domain of electromagnetism, it initiated a very different dialectic process, which brought to the open a deep-seated contradiction between the atomistic and dynamical aspects of electromagnetic phenomena. In the hands of THOMSON and MAXWELL, the dynamical treatment of these phenomena took a peculiar turn away from concrete representation and towards mere formalism: the mechanical constructions ceased to aim at picturing real processes, but became « models » with no other purpose than that of illustrating the compatibility of a set of phenomenological equations with the laws of mechanics. This retreat into abstraction was at first meant to be no more than strategic: it soon became clear that it heralded the end of dynamical philosophy. The electromagnetic field came to be regarded as a primary constituent of the physical world on the same footing as ponderable matter; then, with the discovery of the electron and of the essentially electrical structure of matter, the reversal of the respective

positions of mechanics and electromagnetics was completed: the question was raised whether electric constituents of atoms could not be accounted for in terms of the electromagnetic field alone. The impossibility of fulfilling this programme opened the protracted crisis out of which, through the incorporation of the fundamental quantal features, the modern particle-field synthesis eventually emerged.

4·2. *Faraday.* – At the origin, there was not a shadow of doubt about the concrete meaning of mechanical pictures. FARADAY's astounding power of creative imagination was of the same kind as that of the artist whose searching look discloses the revealing features out of which he composes a portrait presenting a true likeness of his model. The directness of his method has been well characterised by HERTZ [17 *a*]: « a mind of great freshness, who approached the phenomena anew, uncommitted, who started again from what he saw, not from what he had heard, learned or read. » HERTZ then aptly describes FARADAY's conceptions and contrasts them with those of central force physics: « FARADAY heard that when a body was electrified, something is put into it, but he saw that the modifications produced manifested themselves only outside and not at all inside it. FARADAY was taught that the forces simply passed over space, but he saw that it makes all the difference for the force with what matter the allegedly passed-over space is filled. FARADAY read that the existence of electric fluids was certain but that there was some debate about electric forces; and yet he saw how these forces palpably developed their effects, while he could not perceive anything of the electric fluids themselves. Thus the situation was reversed in his mind. The electric and magnetic forces became for him the existing, the real, the palpable things; electricity and magnetism became the things whose existence was debatable. The lines of force, as he called the forces conceived as subsisting by themselves, appeared to his mind's eye in space, as states of space, as tensions, as vortices, as currents, or whatever they might be—this he could not tell—but there they were, influencing each other, pushing and pulling bodies to and fro, and spreading out from point to point, transmitting the excitation. »

With this vivid picture of the forces was coupled in FARADAY's mind the conviction of their fundamental identity and convertibility. It is this belief which decided the orientation of the research which led to his most beautiful discoveries [10]. A striking example is the discovery in 1845 of the rotation of the plane of polarisation of light by a magnetic field [9 *a*]: this phenomenon was the first hint of a deep-lying relationship between optics and electromagnetism, and made a profound impression on both THOMSON and MAXWELL. For FARADAY himself, it was the occasion of remarkable speculations, his « Thoughts on ray-vibrations » (1846) [9 *b*], which reveal us the uncanny precision of his thinking by means of mental pictures of « lines of force ». Of

his general views about the structure of matter we are informed by another short paper [9 *c*], written some time previously (1844): there he conceives ponderable matter as consisting of structureless, punctiform atoms, surrounded by a continuous distribution of their various « powers », which extends from each atom to all the others, not necessarily uniformly in all directions. Now, he considers, more precisely, the atoms as linked together by lines of force along which these powers are transmitted, and he clearly visualises the propagation along such a line of a slight displacement of an atom as a « shake or lateral vibration », i.e. a transversal wave-pulse. The lines of force seem to him more apt to transmit transversal vibrations than a uniform medium like the aether, which he accordingly proposes to « dismiss ».

After this sample of FARADAY's method, it would be very superficial indeed to represent him as a pure « experimental philosopher », in opposition with the mathematically minded physicists of the French type. His strength lies indeed in his perfect awareness of the value and limitation of theoretical thinking. He always readily expressed the theoretical ideas in which he constantly sought guidance in his experimenting. As to speculations which could not directly be founded on experiments, he was naturally more reticent; but after all, he *did* publish the two papers just mentioned, and even though the occasion which prompted the second one was accidental [11], the writing-up of the papers, at any rate, was deliberate: of the first one he explicitly declares [9 *c*] that it would « serve as the record of my opinions and views, as far as they are at present formed ». Moreover, he created his own set of theoretical concepts of a topological and dynamical nature, such as that of « line of force », which fulfilled in his reasoning the same function as more conventional mathematics. In fact, as MAXWELL says, the virtuosity with which he operated with these representations stamps him as « a mathematician of a very high order » [14 *a*].

MAXWELL's relationship to FARADAY is similar to that of LORENZ to ORSTED: when in 1854, at the age of 23, MAXWELL turned his attention to the subject of electricity, FARADAY, 40 years his senior, had accomplished his life's work: his ideas, embodied in the monumental series of his *Experimental researches in Electricity*, had received their final formulation. MAXWELL set himself the task [15 *a*] to translate them into the usual mathematical language in order to investigate their precise connexion with the electrodynamical theories based on action at a distance, and to give them, if possible, a firm foundation upon mechanical principles. Some letters he exchanged with FARADAY after the publication of his first paper (1857) [16 *a*, *b*, *c*] show how eager the old master was to respond to the attempt of a fellow-physicist to enter into his modes of thinking, and how touchingly he gave expression to a satisfaction mingled with awe that his conceptions should have stood so well the « mathematical force made to bear upon » them [19 *a*].

4·3. *William Thomson.* – THOMSON's influence upon MAXWELL's thought, hardly less profound than that of FARADAY, was of a different character. The two men, similarly distinguished by the precocity and originality of their talent, equally moulded by cantabrigian education, with a difference of age of only seven years, entertained personal relations of easy cordiality, which, however, never went very deep. THOMSON, appointed to the Glasgow chair at the age of 22, had soon acquired so much authority in the field of electricity and magnetism that it was to him that young MAXWELL turned for advice [13 *a*] as soon as he had passed the ordeal of the Tripos examination. A year later (1855), we see MAXWELL seeking [13 *b*] and obtaining [16 *d*] THOMSON's permission « to poach on his electrical preserves ».

THOMSON's first paper on the subject [12 *a*] is indeed one of the most extraordinary productions which have left their mark in the history of science. He wrote it at the age of 17, in the summer of 1841, just before he took residence in Cambridge (*). In the immature style of a schoolboy's exercise, it presents in full generality the fundamental theorems of the « theory of attraction » (now called potential theory) with their application to the case of ellipsoids. It is true that the schoolboy had been forestalled by CHASLES and by GAUSS, and these in turn, as was later discovered, by GEORGE GREEN; but more astonishing than the results was the method by which they were obtained—a method which is entirely THOMSON's. It is based on the formal analogy between potential theory and that of stationary heat flow. Although the name « potential » was not yet in use, the concept itself and the differential equation which characterises this quantity were well-known since LAPLACE; and FOURIER had shown that the theory of heat, in the stationary case, was governed by the same equation. THOMSON now makes use of the correspondence between equipotential and isothermal surfaces, and between the derived quantities, electric intensity and heat flow, in order to deduce the general properties of electrostatic forces and charge distributions from those of heat flows and distributions of temperature. The latter he regards as « obvious » to physical intuition; about two years later, in a letter to his father [13 *d*] (°), he

(*) The otherwise reliable biography by S. THOMPSON [13*c*] is in error on this point: it mentions the date of August 1842 instead of 1841. The date of publication in the *Cambridge Mathematical Journal* is February 1842.

(°) Lest anybody should think that he received any inspiration from the Cambridge environment, I may just mention that in the same letter he quotes his tutor as « hoping I do not now lose any time with the *Math*[*ematical*] *Jour*[*nal*] »! These were the dons who subjected the self-taught genius GEORGE GREEN, at the age of 44, to the torture of the Tripos, and kept his great *Essay* unread on their shelves. Equally edifying is the story [25] of how, some years later, the (figuratively) deaf and blind astronomer CHALLIS managed to miss the planet Neptune at the place correctly predicted by the young graduate JOHN COUCH ADAMS.

describes them, with due emphasis, as « propositions, deduced *entirely from physical considerations* »: only then was he able to give analytical proofs of them [12 *b*].

In order to appreciate the novelty of this juvenile onslaught upon the problems which had occupied the deepest minds of the French school—Poisson, Gauss and Green, to mention only the greatest,—it must be realised that the two monumental works of this school, Laplace's *Mécanique Céleste* and Fourier's *Théorie analytique de la Chaleur*, had peacefully co-existed for twenty years without anyone attempting to follow up the mathematical analogy in question. We witness here an effect of that tendency to metaphysical rigidity which was beginning to hamper the development of mechanistic thought: Fourier, stressing the essential difference between the analytical character of the law of heat conduction (in the general case) and those of mechanics, peremptorily represented this difference as for ever irreducible, independently of any hypothesis about the nature of heat [2].

One may imagine, therefore, the sensation which the young Cambridge graduate created among the Paris physicists when he arrived, in 1845, bringing with him on the one hand Green's *Essay* (which he had just then been able to see for the first time (*)), on the other his own productions. Green's work was the logical completion of the analytical treatment of central forces in the pure Laplacian tradition. But Thomson's derivation of the same results from a formal analogy with the theory of heat conduction had much farther-reaching implications: it opened the prospect of a possible physical analogy on the lines suggested by Faraday's discoveries. In a paper [12 *c*] written in Paris, at Liouville's request [13 *f*], Thomson developed with great vigour this aspect of the problem, emphasising the heuristic superiority of Faraday's views and the strong corroboration they had received from the discovery of the influence of the dielectric medium on the electrostatic forces. « It is, no doubt, possible » he wrote « that such forces at a distance may be discovered to be produced entirely by the action of contiguous particles of some intervening medium, and we have an analogy for this in the case of heat, where certain effects which follow the same laws are undoubtedly propagated from particle to particle ».

The following year marked a further significant progress in Thomson's approach to a dynamical theory of electro-magnetism. Deeply impressed by Faraday's discovery of the action of a magnetic field upon light traversing a transparent solid, he now looked for an analogy between electromagnetic situations and states of strain in an elastic body. His friend Stokes had just given (1845) the equilibrium conditions for such a body (assumed iso-

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(*) See the story of the « discovery » of Green's *Essay* in S. Thompson's biography [13*e*].

tropic) in the form of a differential equation for the displacement vector $\boldsymbol{u}$:

$$\Delta \boldsymbol{u} = \operatorname{grad} \theta\,, \qquad \text{with} \qquad \theta = -k \operatorname{div} \boldsymbol{u}\,;$$

the latter equation, in which k is proportional to the compression modulus, reducing for an incompressible solid to $\operatorname{div} \boldsymbol{u} = 0$. In a laconic note [11 *d*], published in 1847, THOMSON points out that these equations have indeed particular solutions which exhibit the characteristic properties of electrostatic, magnetostatic and electrodynamic quantities. If $\boldsymbol{r}$ denotes the vector from some fixed point O to the point of space considered, r the corresponding distance and $\boldsymbol{m}$, $\boldsymbol{j}$ constant vectors, these solutions and their analogies are as follows:

$\boldsymbol{u} = -\operatorname{grad}(1/r)\,,$	electrostatic field of a unit point charge at O;
$\boldsymbol{u} = \operatorname{rot}(\boldsymbol{m}/r)\,,$	vector potential of a magnet of moment $\boldsymbol{m}$ at O;
$\boldsymbol{u} = \boldsymbol{j}/r - \tfrac{1}{2}\operatorname{grad}(\boldsymbol{j}\cdot\boldsymbol{r}/r),$	vector potential of an element of current of density $\boldsymbol{j}$ at O.

There is nothing in THOMSON's brief paper to disclose the depth of his searching or the hopes he entertained. His notebooks [13 *g*], however, are extremely revealing in these respects: We see him grappling for nearly a month (November 1846) with the then quite new subject of elasticity before he could express in formulae his intuitive guesses. One must not forget that the concepts of vector analysis, so familiar to us, were then utterly unknown and had to be constructed in the very process of development of the new physical ideas. The final entry leaves no doubt about his expectations: « I have at last succeeded in working out the *mechanico-cinematical* (!) representation of electric, magnetic and galvanic f^{ces}... ». Later (1847), in a letter to FARADAY [13 *h*], commenting upon his paper, he shows himself as clearly aware of the preliminary character of his results as of the ultimate aim to be achieved: « a physical theory of the propagation of electric and magnetic forces ».

5. – Maxwell.

> I have also a paper afloat, with an electromagnetic theory of light, which, till I am convinced to the contrary, I hold to be great guns.
>
> J. C. MAXWELL (letter of 5 Jan. 1865) [16 *g*]

5·1. *Hydrodynamical analogies.* – It is at this stage that MAXWELL takes up the problem, in 1855. Not that THOMSON had been idle in the intervening years; he had written voluminous treatises in which he attempted a more systematic treatment of electricity and magnetism, embodying FARADAY's

discoveries of the influence of the material media, dielectric, diamagnetic or paramagnetic: these are very heavy and, although of great value as pioneering work, lack the inspiring suggestiveness of the preceding short notes; but it was from them that MAXWELL acquired the mathematical preparation for his own investigations. Moreover, in those critical years, during which the whole science of thermodynamics was created, THOMSON had devoted a still larger part of his amazing activity to this new and exciting task. His eager curiosity and quick intelligence made him a sower of ideas in many fields; his inconstancy too often prevented him from reaping the harvest. So it was with thermodynamics, where he was forestalled by CLAUSIUS; so it was to be with electromagnetism, where he abandoned to MAXWELL the promising quest.

MAXWELL's first effort [14 *b*] was still of limited scope, though no longer, like THOMSON's work, confined to static or stationary phenomena; he also discussed electromagnetic induction, but his treatment of electric currents could only claim validity for the case of closed circuits. In the first part of the paper, MAXWELL goes again over the same ground as THOMSON: electrostatics, magnetostatics and stationary currents, but he follows a new analogy, that of a flow of incompressible fluid in a resisting medium. MAXWELL's treatment of the dynamical analogy is characteristically different from THOMSON's: to the latter's analytical power he adds a fine geometrical intuition, which allows him to present the whole discussion in a simple and straightforward language, from which all but a few algebraic formulae are banished. This is indeed one of the great papers of MAXWELL's, which remain as unsurpassed models of elegant exposition and of beautifully simple and clear scientific English (*). The hydrodynamical analogy is the natural one to illustrate FARADAY's concept of « line of force », and this was MAXWELL's main purpose in this work. If $\boldsymbol{v}$ is the velocity of flow of an incompressible fluid of unit density, and if the resisting force per unit volume is assumed to be of the form $R\boldsymbol{v}$, the equations describing the stationary situation are

$$\operatorname{div} \boldsymbol{v} = 0 , \qquad R\boldsymbol{v} = \operatorname{grad} p ,$$

the latter expressing the fact that the resistance of the medium is balanced by the pressure gradient. MAXWELL follows FARADAY in treating the three main groups of electric, magnetic and « galvanic » phenomena in a completely parallel way: he applies to all of them the same hydrodynamical picture.

(*) Every young physicist with any ambition to culture ought to have read this paper, as well as the second one, described below. This can be done in an hour or two; thus no one may excuse himself by pleading that it would take up too much of his valuable time.

In each case, he distinguishes two characteristics, a « quantity » and an « intensity », linked by a relation of proportionality, whose factor corresponds to a « resistance » of the material medium; the « quantity » obeys the solenoidal condition (except at sources or sinks), while the « intensity » appears as the gradient of a « pressure » or « tension ». The triple analogy is summarised in the following Table:

	Quantity	Intensity	Inverse of resistance
Electrostatics	« inductive action » $\boldsymbol{D}$	electric intensity $\boldsymbol{E}$	« specific inductive capacity » ε
Magnetostatics	« induction » $\boldsymbol{B}$	magnetic intensity $\boldsymbol{H}$	« magnetic conductive power » μ
Electrodynamics	current density $\boldsymbol{j}$	e.m.f. $\boldsymbol{E}_{el}$	electric conductivity σ

The distinctive success of MAXWELL's approach, however, is the interpretation of FARADAY's « electro-tonic state », which he gives in the second part of the paper. According to FARADAY, the induction of a current in a closed conductor suggests that this conductor, in the presence of a magnetic field, is in a peculiar state, which does not give rise to observable effects so long as conditions are stationary, but whose changes are revealed by the occurrence of an electromotive force in the conductor. In order to translate this conception in mathematical terms, MAXWELL has recourse to an argument of a type quite novel at that time: HELMHOLTZ had just shown (in his famous essay of 1847) how the law of electromagnetic induction could be derived from those of stationary currents by considering the transformations of energy involved in the process and applying the conservation law. MAXWELL repeats the argument, but on the basis of new expressions for the magnetic energy and the JOULE heat, consonant with the view of a continuous distribution of this energy in space. In modern notation, the equation he sets up for the energy changes during a time δt when the magnetic induction is varied by $\delta \boldsymbol{B}$ may be written

$$\delta t \int \boldsymbol{j} \cdot \boldsymbol{E} \, \mathrm{d}v + \frac{1}{4\pi} \int \boldsymbol{H} \cdot \delta \boldsymbol{B} \, \mathrm{d}v = 0 \, .$$

Now, expressing the induction as $\boldsymbol{B} = \mathrm{rot}\, \boldsymbol{A}$ by means of the vector potential $\boldsymbol{A}$,

and using AMPÈRE's law written in its local form (*)

$$\text{rot}\, \boldsymbol{H} = 4\pi \boldsymbol{j}\,, \tag{12}$$

the condition of energy conservation is transformed into

$$\int \boldsymbol{j} \cdot \left(\boldsymbol{E} + \frac{\delta \boldsymbol{A}}{\delta t} \right) \mathrm{d}v = 0\,: \tag{13}$$

the variation of the induction flux in the space embraced by the conductor has been replaced by the variation of the quantity $\boldsymbol{A}$ *inside* the conductor; and from the validity of the equation (13) for arbitrary dimensions of the conductor and magnitude of the current density, one derives the local equation

$$\boldsymbol{E} = -\frac{\partial \boldsymbol{A}}{\partial t}\,, \tag{14}$$

immediately suggesting the identification of the vector potential with the electro-tonic state visualised by FARADAY.

5·2. *Vortices and idle wheels.* – MAXWELL does not at all regard this conclusion as a satisfying answer to the question; in fact, he is quite apologetic about its abstract character: there is as yet no clear dynamical picture of the link which must exist between electric and magnetic lines of force. How deeply he had assimilated the conception of physical line of force is shown by an interesting letter to FARADAY [16 *c*] of 1857, in which he applies it to the interpretation of gravitation: in this case, the lines must « have a *pushing* force instead of a *pulling* one », a feature which greatly impressed FARADAY [26 *b*], and baffled MAXWELL completely when he returned to the subject later [14 *e*]. As to the problem of electromagnetism, he seems to have expected further light from the analogy, already noticed by THOMSON, with elastic solids or (what formally amounts to the same) viscous fluids. Very soon, however, another brilliant remark of THOMSON led his thoughts into rather different channels. In 1856, THOMSON [12 *d*, 15 *b*] returned to the effect of a magnetic field on the velocity of propagation of circularly polarised light and discussed it from the then unfamiliar point of view of its mirroring transformation properties: he came to the conclusion that the circular « luminiferous » motion must be compounded with another circular motion linked in a definite way with the magnetic field.

THOMSON's paper, as usual, is verbose and far from clear, but the hint it contained was enough to give MAXWELL the right start. The decisive point

(*) In electromagnetic units.

was that one had for the first time an indication from experimental fact sufficiently definite to narrow down the range of possible mechanical analogies to a considerable extent. Working progressively from the representation of magnetic tubes of force as vortices to its successive implications about the nature of electromagnetic induction, electric current and electric polarisation, MAXWELL built up piece by piece this startling machinery of the electromagnetic aether, which eventually disclosed to his wondering gaze the long-sought mechanism of propagation of a purely transversal disturbance like that of light. No trace has been left, unfortunately, of the progress of MAXWELL's thought in these years: we have only the final description of the perfected work in his second great paper [14 *c*] of 1861-62. This period was one of the most active in MAXWELL's life: we see him carrying out his monumental investigation of Saturn's rings, which fully occupied the years 1857-59, starting his researches in the kinetic theory of gases (1860) and completing his theory of colours (1860). In the published correspondence, the only allusions to electromagnetism date from 1861 and 1862: it is conceivable that the whole picture, intricate as it is, may have crystallised in his mind in the course of a few months.

A vortex-tube of axial symmetry gives rise to a system of stresses which can be resolved into a lateral pressure p and a longitudinal pressure or tension of the form $C\varrho v^2 - p$, where ϱ ad v are the mean density of the fluid and its velocity at the boundary of the tube, and the numerical constant C depends on the distribution of density and angular velocity, taking the value $\frac{1}{4}$ if this distribution is uniform. It is readily seen that this tensor reproduces the known forces of magnetic origin, provided that the magnetic intensity $\boldsymbol{H}$ is interpreted as a vector of magnitude r directed along the axis of the tube (i.e. proportional to the angular velocity of the vortex) and the magnetic permeability μ is identified with the quantity $4\pi C\varrho$. The density of magnetic poles appears as $(1/4\pi)$ div $\boldsymbol{B}$ and the density of (stationary) current as $(1/4\pi)\cdot$ $\cdot$rot $\boldsymbol{H}$. As to the pressure p, MAXWELL leaves it unspecified; to fix it, it would be necessary to consider in more detail the magnetisation of the medium.

After this merely descriptive picture of the magnetic field as a distribution of vortices, MAXWELL takes up the dynamical problem of how these vortices are set in motion and arrange themselves according to the laws of electromagnetism. This is the first time in the history of the search for mechanical analogies that the crucial part of such an analogy, its dynamical aspect, is developed. The very strangeness of MAXWELL's solution illustrates the difficulty of the problem. To account for the transmission of rotation *in the same sense* from vortex to vortex, MAXWELL conceived that neighbouring vortices are separated by a layer of particles capable of rolling upon the surfaces of the vortices, like « idle wheels »: the translation of such a layer of « idle wheel » particles is the image of the electric current. In fact, one finds that, under

stationary conditions, the flow of particles is proportional to rot $\boldsymbol{H}$, while the interplay of vortices and particles imitates all the features of electromagnetic induction. From energy considerations, the electromotive force $\boldsymbol{E}$ acting upon the particles is found to be connected to the change of the vortex motion by the equation

$$\text{rot}\, \boldsymbol{E} = -\frac{\partial \boldsymbol{B}}{\partial t}\,; \tag{15}$$

i.e. it is expressed in terms of the vector potential by eq. (14): the e.m.f. appears as a mechanical force resulting from a gradual change in the rotation of the vortices; the electrotonic state corresponding to the impulse which would arise from a sudden stoppage of this rotation.

We thus picture the medium transmitting electromagnetic actions as an agglomeration of « cells » capable of rotation, separated by layers of « idle wheel » particles, which form the substance of electricity. The last step is now to visualise more concretely the mechanism of interaction between the cells and the particles. Guidance is here afforded by the behaviour of material bodies under a gradient of electric potential, which simply corresponds to a pressure gradient in the layer of electric particles. If we imagine the structure of cells and intervenig layer to be on a finer scale than the molecular structure of matter, we may conceive electric conduction, in such a pressure gradient, as the passage of particles from molecule to molecule, only hampered by some resistance; but in a dielectric, the displacement of the particles appears to remain confined within each molecule, which thus becomes polarised. This strongly suggests that some elastic mechanism comes into play: MAXWELL assumes that the substance of the cells yields to some extent to the tangential action exerted by the particles when they are displaced; this distortion of the cells calls forth an elastic reaction, which, if the initial difference of potential is removed, acts as an equal and opposite electromotive force upon the particles. The relation between the displacement $\boldsymbol{D}$ and the electromotive force $\boldsymbol{E}$ which produces it is accordingly of the form

$$\boldsymbol{E} = 4\pi c_1^2 \boldsymbol{D}\,; \tag{16}$$

the constant c_1 is a function of the elastic parameters of the cellular substance, depending on the structure of the cells.

The consideration of the electric displacement at once brought MAXWELL to the threshold of his greatest discovery. « This displacement » he observes « does not amount to a current, because when it has attained a certain value it remains constant, but it is the commencement of a current, and its variations constitute currents. » The discharge of a condenser is a typical example of the production of such a displacement current: as the strain on the cells is

released, their elastic reaction moves the particles back to their original positions. The displacement current $\partial \boldsymbol{D}/\partial t$ is of the same kind as the conduction current and has to be introduced into the equation connecting currents and magnetic vortices; one has accordingly, instead of (12) (*),

$$\boldsymbol{j} + \frac{\partial \boldsymbol{D}}{\partial t} = \frac{1}{4\pi} \operatorname{rot} \boldsymbol{H} . \tag{17}$$

Now, an argument similar to that underlying WEBER's comparison of the electrostatic and electromagnetic units of charge leads to a very simple interpretation of the constant c_1. The equation of continuity, combined with eq. (17), shows that the charge density, in electromagnetic units, is just given by $\operatorname{div} \boldsymbol{D}$. In the static case the corresponding energy, computed as the work done by the e.m.f. $\boldsymbol{E}$ in the displacement $\boldsymbol{D}$, is $\frac{1}{2}\int \boldsymbol{E}\cdot\boldsymbol{D}\,\mathrm{d}\tau$, i.e. on account of eq. (16), c_1^2 times the familiar expression for the electrostatic energy of a charge distribution of density ϱ, in air, $\frac{1}{2}\int \varrho_{\mathrm{P}}\,\mathrm{d}\tau_{\mathrm{P}}\,\varrho_{\mathrm{P}'}\,\mathrm{d}\tau_{\mathrm{P}'}/r_{\mathrm{PP}'}$; this shows that the value c of the constant c_1 in air precisely represents the ratio of the two units of charge.

This ratio is thus connected in a definite way with the elastic properties of the cellular substance, and more particularly, since the mechanism devised by MAXWELL involves tangential stresses, with its torsion modulus: it is thus closely related to the velocity of propagation of transverse vibrations in this substance. MAXWELL now proceeds to determine this relation by specifying in an arbitrary way the properties of the cell substance: he assumes that the cells are of spherical shape and have the elastic properties of a solid of the Navier type; this gives him a definite value for the torsion modulus in terms of the constant c^2. Moreover, he needs the density of the cells, to the inverse of which the square of the propagation velocity is proportional; to this end, he fixes the constant C connecting the density with the magnetic permeability of the material medium at the value $\frac{1}{4}$ corresponding to uniform conditions. Then, in working out the velocity of propagation, he inadvertently omits a factor $1/\sqrt{2}$ and by sheer luck obtains for this velocity the correct value $c/\sqrt{\mu}$!

We have unambiguous evidence that this is indeed the extraordinary way in which he arrived at the momentous conclusion that « light consists in the transverse undulations of the same medium which is the cause of electric and magnetic phenomena. » A letter of October 1861 has fortunately been preserved [16 *e*], in which he announces to FARADAY the results he has just ob-

(*) In MAXWELL's paper, the displacement occurs with the opposite sign in the equations equivalent to (16) and (17): this is because he analyses the « discharge » process, but counts the displacement from the position of the particles in the undisturbed state.

tained: and we find there the whole above argument plainly stated in non-mathematical language. After giving in miles per second the calculated value of the velocity of propagation and quoting for comparison FIZEAU's determination of the velocity of light, he adds the interesting comment: « This coincidence is not merely numerical. I worked out the formulae in the country before seeing WEBER's number, which is in millimeters, and I think we have now strong reason to believe, whether my theory is a fact or not, that the luminiferous and the electromagnetic medium are one. »

MAXWELL's train of thought at this stage would seem to have been about the following: to visualize the transmission of electromagnetic effects we need a substance possessing transverse rigidity, like the luminiferous aether; if we could show that by assigning to it reasonable elastic properties (such as those suggested by NAVIER on an atomistic basis) we could make it transmit transverse disturbances with a velocity not merely proportional but equal to that of light, we should get a very strong presumption of the identity of the two media. The conviction that this must certainly be possible, even in more than one way, may account for the apparently negligent way in which he carried out the calculation.

Anyhow, MAXWELL had then not yet explored to any great extent the consequences of the identification of the electromagnetic and luminiferous media. The only point he discusses both in the paper and in the letter to FARADAY is the influence of dielectric matter on the velocity of propagation; this is another consequence of the fundamental eq. (16) and of the relation $\varrho = c \operatorname{div} \boldsymbol{D}$ derived, as we have seen, from the idea of displacement current. From these equations follows the relation $-4\pi\varrho = (c^2/c_1^2)\Delta V$ between charge density and electrostatic potential, which shows that the specific inductive power of the dielectric ε is given by the ratio c^2/c_1^2 of the squares of the velocities of propagation in air and in the medium, respectively; or if n denotes the refractive index, one has between the electromagnetic and optical parameters of the medium the relation $\varepsilon = n^2/\mu$: a formula of great practical importance, as it opens the possibility of an extensive experimental test of the theory.

MAXWELL was guided to his conception of electric displacement and displacement current by such concrete phenomena as the polarisation of a dielectric and the discharge of a condenser; but it is interesting to observe how readily he took the decisive step of making such displacements independent of the presence of ponderable matter. It would not be right to say that he treated the aether as any ordinary material medium; but, imbued as he was with FARADAY's ideas, he regarded all material bodies as essentially composed of the same medium, modified only by the molecules of ponderable matter somehow embedded in it. More and more distinctly he perceived that the whole variety of electromagnetic and optical phenomena were but manifesta-

tions of a complicated interplay of elastic stresses and strains in a gigantic, all-pervading mechanism.

5·3. *Dynamical models.* – MAXWELL's second paper marks the climax of the dynamical conception of nature: in it this conception reached its highest achievements; in fact, as soon became apparent, it overreached itself. In 1864, MAXWELL published a third paper [14 *d*], entitled *A dynamical theory of the electromagnetic field*; beautiful as it is, it comes as an anticlimax: after the soaring flight of creative imagination, the sober and sobering searchings of criticism. There is no longer any question of indulging in details of ingenious transmission mechanisms; all that is given is a careful enumeration, in logical order, of the essential concepts and equations of the theory, interspersed (this is typical of MAXWELL's turn of mind) by practical applications worked out in every detail. As an exposition of the abstract logical framework of the theory, this paper is of supreme elegance; but the contrast with the concrete character of the preceding one is all the more striking. The programme of giving a dynamical account of electromagnetic phenomena is by no means abandoned (as indeed the title indicates), and the whole first part of the paper is devoted to an admirable summary in ordinary language of the mechanical ideas of the previous paper; what is left out is the concrete image of cells and particles with their peculiar elastic linkage. Certainly, MAXWELL had already in his second paper explicitly disclaimed for this « somewhat awkward » conception any intention of describing « a mode of connexion existing in nature »; but he had firmly asserted its value as a « conceivable, and easily investigated » one, serving « to bring out the actual mechanical connexions between the known electro-magnetic phenomena [14 *f*]. » Its abandonment is not merely the discarding of a scaffolding after the completion of the building; it marks a decisive turning of the road leading away from dynamical philosophy.

It is not difficult to conjecture what happened. MAXWELL, as we would expect, has by now attained full clarity about the foundation of the electromagnetic theory of light. As he expresses it in a letter of September 1864, he has « cleared [it] from all unwarrantable assumption [16 *f*] »: he has seen how to base it on the field equations alone, without any appeal to the theory of elasticity. The derivation he gives of the wave-equation is rather clumsy, but the essential points emerge: transversality of the vibrations, unique value of the velocity of propagation. Some time later, when he wrote his last great electrical paper (1868) [14 *h*], Maxwell's grasp of the theory was so thorough that he was able to crystallise its whole content in just a few pages of admirable lucidity; it is there, in particular, that its characteristic feature is for the first time explicitly expressed in the statement that all electrical circuits are closed. It was especially important to recognise the fact that the velocity of propa-

gation comes out as $c_1/\sqrt{\mu}$, independent of the elastic structure of the aether: this structure is only represented by the constant $4\pi c_1^2$ which connects electromotive force and displacement, and which can directly be related to the ratio of the units of charge, determined « by measuring the attraction between bodies kept at a given difference of potential, the value of which is known in electromagnetic measure [16 *f*] ». Just at that time, new determinations of the unit ratio were being carried out on this very principle (suggested by THOMSON [13 *i*]) as part of the programme of the British Association Committee on Electrical Standards, and they provided (cf. the adjoining table) a welcome confirmation of the result of WEBER and KOHLRAUSCH: MAXWELL himself performed one of these new measurements, which he described in his 1868 paper [14 *h*].

Determinations of c	($\cdot 10^{10}$ cm s^{-1})
Ratio of electrical units	
WEBER and KOHLRAUSCH (1856)	3.11
MAXWELL (1868)	2.88
THOMSON (1869)	2.93
Velocity of light	
Astronomical (from aberration)	3.08
FIZEAU (1849)	3.15
FOUCAULT (1862)	2.98

But just as the theory gained a firmer empirical foundation and a neater formal structure, the very definiteness and universality of its enunciations brought sharply to the fore the difficulty of reconciling it with any elastic theory of the propagation of light. Thus, the fact that only transversal vibrations are propagated by the electromagnetic medium at once conjured up the unsolved riddle of how to dispose of the longitudinal vibrations inherent in elastic disturbances of the ordinary type. It is significant, in this respect, that MAXWELL did not succeed in giving a derivation of the laws of reflexion and refraction—the *crux* of all elastic theories: a letter to STOKES of 1864 reveals his unsuccessful grappling with this problem [19 *a*]. He thus came to recognise that the difficulties besetting theoretical optics had now to be faced just as well in electromagnetics; in the paper of 1864 he admits as much in the telling sentence: « both sciences are at a loss when called on to affirm or deny the existence of normal [i.e. *longitudinal*] vibrations [14 *g*] ».

It is clear, therefore, that MAXWELL felt himself in the grip of an acute dilemma. The conviction of the real existence, in a concrete physical sense, of an underlying mechanism of transmission of electromagnetic actions had

led him to the discovery of the identity of this mechanism with that of light propagation; and now, the very unfolding of the logical consequences of this discovery, far from helping him to elucidate the mechanism, was throwing him back against the same obstacles which had so long hampered progress in optics. The course he took in this quandary was undoubtedly a wise one: he stressed those features of the theory which were independent of all uncertainties about the underlying mechanism, and renounced all explicit consideration of the latter. Like NEWTON or AMPÈRE before him, MAXWELL did not see in this kind of renunciation anything but the temporary laying aside of a difficult problem, unripe for fruitful discussion. Yet we can understand THOMSON, the staunch believer in the dynamical nature of ultimate reality, shaking his head and regarding the electromagnetic theory of light as a backward step [13 *j*]. He made his meaning quite clear in one of the Baltimore lectures [13 *k*]; « I firmly believe in an electromagnetic theory of light, and that when we understand electricity and magnetism and light we shall see them all together as parts of a whole. But I want to understand light as well as I can, without introducing things that we understand even less of. That is why I take plain dynamics. I can get a model in plain dynamics; I cannot in electromagnetics. » THOMSON was in a way of reactionary temperament, clinging to the last to the ideas he had conceived in his youth; but he cannot be denied clearsightedness in his judgment of the trend of ideas in physics. In this case, if he opposed the change of emphasis from the equations of dynamics to those of electromagnetism, introduced in MAXWELL's third paper, it was because he foresaw more clearly than anybody else where it would ultimately lead.

The word « model » in THOMSON's remarks just quoted concentrates in itself, so to speak, the whole problem of dynamical philosophy. Mechanical analogies of the kind considered by THOMSON and MAXWELL are indeed in the first instance *models* intended to imitate certain physical effects whose dynamical nature is not immediately apparent, and thereby reduce these effects to a more familiar form. In this function, they are an aid to thinking, they have heuristic value. But this is by no means their only, or even their essential epistemological significance: in the search for dynamical models is implied the postulate that such models can always be found, because all phenomena are fundamentally dynamical in nature. In other words, dynamical models are not imitations of things non-dynamical, but attempts to reproduce, from the observation of their effects, the features of real dynamical systems. Dynamical models are thus meant to be images of reality, the increasing quality of which will be judged by their ability to account for a larger and larger number of the observed phenomena. Let us not scorn this ideology: should we be tempted to find it crude or naive, let us remember what MAXWELL achieved with his vortices and idle wheels—and bow with reverence.

Yet, MAXWELL is perhaps greater still in our eyes because in this dynamical symphony his critical ear perceived a jarring note. His efforts to fix the really essential features of the elusive dynamical picture he was trying to outline led him eventually to realise that such a picture was in fact left largely undetermined by the conditions of the problem. The derivation of the field equations given in the paper of 1864 starts from the consideration of a system of closed circuits, each characterised by a single variable (the current intensity). The electrodynamical laws are first derived in integrated form, and then reduced to the local form by the now familiar procedures; the idea that all currents are closed when variations of displacement are taken into account makes the argument perfectly general. Now, in this reasoning, the analysis of the system of coupled circuits was still compared to a special « dynamical illustration », referring to the reduced momentum of a fly-wheel. But in the *Treatise* of 1873, which embodies the latest state of MAXWELL's electromagnetic meditations, the argument is presented [15 *c*] in a purely abstract manner, based on the Lagrangian formalism of point dynamics. What is here shown is thus that the laws of electromagnetism are compatible with those of dynamics, and consequently allow of an infinity of mechanical models, all fulfilling their function equally well. Any particular one will play the part of an « existence proof » just as well as any other. This remarkable conclusion is formulated with all possible clarity in a later part of the *Treatise* [15 *b*] (every feeling ranging from reproach to regret has often enough been expressed regarding the bad composition of this wonderful book!); here it is in full: « The attempt which I then [in 1862] made to imagine a working model of this mechanism must be taken for no more than it really is, a demonstration that mechanism may be imagined capable of producing a connexion mechanically equivalent to the actual connexion of the parts of the electromagnetic field. The problem of determining the mechanism required to establish a given species of connexion between the motions of the parts of a system always admits of an infinite number of solutions. Of these, some may be more clumsy or more complex than others, but all must satisfy the conditions of mechanism in general ».

The recognition of the existence of an infinity of possible mechanical models of electromagnetic phenomena and of their essential equivalence was a death blow to any pretension of deriving from mechanical considerations a faithful picture of physical reality. At the end point of the eventful evolution of MAXWELL's thought, we see fully vindicated the view of the essential ambiguity of the aether conception, held long before by LORENZ. No doubt, the ideal of an ultimate mechanical explanation of all natural phenomena will still linger on for some time. BOLTZMANN, who was as unflinching an exponent of this philosophy as THOMSON, will still ask, in the peroration of his magnificent speech *On the development of the methods of modern theoretical physics* [18] at Munich in 1899: « Will the conviction ever arise that certain pictures are such

that they cannot be superseded by any simpler, more comprehensive ones, that they are "true"...? » HERTZ himself begins his book on the principles of mechanics, in 1894, with the sentence [17 *b*]: « All physicists agree that it is the task of physics to reduce the natural phenomena to the simple laws of mechanics. » But while paying lip-service to a scientific philosophy which had accomplished its historical part, HERTZ was undermining its last positions in electromagnetic theory, by carrying to its ultimate consequences MAXWELL's criticism of the mechanical models of this theory.

6. – Hertz.

HERTZ cannot properly be described as a disciple of MAXWELL: if he appears to pursue a line of development initiated by the latter, it is by no means as a result of a direct filiation of ideas. HERTZ was introduced to MAXWELL's equations by his teacher HELMHOLTZ, a natural philosopher steeped in the modes of thinking of central force physics. Among the Continental physicists, HELMHOLTZ had the great merit of recognising at once the importance of MAXWELL's conceptions; but in laying on them a rather heavy hand he completely spoiled their subtle harmony. What he did was to construct a general scheme of electrodynamics, from the point of view of action at a distance, which would embody all possible laws of electrodynamic action leading to the same result for closed circuits, and clearly show wherein they could differ in their predictions regarding open circuits. In order to compare MAXWELL's formulation with the other versions of electrodynamics, he tried to cast it into the same mould: not only was such an approach to MAXWELL's theory entirely alien to its spirit, but it tended to obscure its characteristic features (*) and to make the theory appear as a somewhat singular limiting case of the scheme.

In his first contribution to electromagnetic theory (1884) [17 *d*], we see HERTZ attacking the problem from the same angle, but with a strong bias in favour of MAXWELL's theory. He gives an ingenious derivation of MAXWELL's equations from « premises which are accepted also by the rival electrodynamics,

(*) This is clearly set out by HERTZ himself in the introduction to his collected papers (1891) [17*c*], in which he compares HELMHOLTZ' and MAXWELL's treatment of dielectric media. HELMHOLTZ represents the polarisation of the dielectric in a condenser as the result of the action at a distance of charges coating the plates of the condenser: each « molecule » is thus charged positively on the side turning towards the condenser plate at the *lower* potential. For MAXWELL, the electric displacement removes all charge from the condenser plates and brings it into the dielectric: the net result is thus that each « molecule » appears positively charged on the side turning towards the condenser plate at the *higher* potential.

and using a chain of reasoning familiar to this electrodynamics »; and he presents it as a compelling argument for the uniqueness of the MAXWELL scheme, vindicating RIEMANN's and LORENZ' pioneering efforts. Nevertheless, when he discovered the electromagnetic waves, he discussed his experimental results in terms of HELMHOLTZ' ideas, and only afterwards, in his famous paper [17 *e*] of 1888, did he show how they could be interpreted on the basis of MAXWELL's theory. Even so, he still betrays in this last paper a total ignorance of MAXWELL's dynamical ideas: he asserts that « MAXWELL arrived at his theory by going out from action at a distance and attributing to the aether the properties of a highly polarizable dielectric medium ». This is in fact an apt description of the point of view adopted by HELMHOLTZ, and only shows that HERTZ had not taken the trouble to read the papers which preceded MAXWELL's *Treatise*. It is not surprising, therefore, that in the systematic exposition of the electromagnetic theory which he gave in 1890 [17 *f*], and which is still the one currently adopted in Germany, only the formal skeleton of MAXWELL's theory subsists: as HERTZ himself declares [17 *e*]: « To the question "What is MAXWELL's theory?" I would not know any shorter and more definite answer than this: MAXWELL's theory is the system of MAXWELL's equations ».

This oft-quoted statement must be understood in the spirit of phenomenology in its constructive, scientific aspect, for which KIRCHHOFF had set an example among the German physicists. HERTZ has by no means forsaken the aether: on the contrary, he visualises it as the primordial substance, whose properties would not only provide the explanation of the electromagnetic and optical phenomena, but would also give the key to an understanding of the essential characteristics of ordinary matter itself: gravity and inertia. This is the programme which he outlines with glowing optimism in his celebrated Heidelberg speech [17 *a*] of 1889. It was not given him to proceed very far with, it, but the work to which he devoted the last years of his brief career (he died in 1894, at the age of 37) was certainly, in his mind, the first step towards an all-embracing theory of aether and matter.

HERTZ' effort in attempting a reformulation of the principles of mechanics [17 *g*] was in fact directed against a logical flaw which had all the time affected the conception of force transmission by a medium itself dynamically described. As MAXWELL, with his usual acuteness, expresses it [15 *d*]: « The observed action at a considerable distance is... explained by means of a great number of forces acting between bodies at very small distances, for which we are as little able to account as for the action at any distance however great ». Obviously, there is no escape from this difficulty except in the complete elimination of the concept of force, and its reduction to that of motion, in accordance with the old cartesian ideal. This is exactly what HERTZ attempts to do: he shows that the way in which dynamical constraints applied to a system of particles affect its motion can always be described as the effect of adding to

the system « hidden » particles in suitably chosen states of motion. As an illustration of what he has in mind, he expressly mentions MAXWELL's mechanical conception of electrodynamic action. HERTZ' thinking was thus here moving along the lines of the British dynamical school. But his profound work remained a torso: HERTZ could not carry it beyond the foundations, and nobody after him ever took it up in the spirit in which it was conceived.

In fact, the spirit of the dynamical tradition, which had achieved so much, was on the wane. The discovery of the electron and the study of fast electrified particles raised more pressing problems, and in this new context, the emphasis put by HERTZ on the phenomenological value of MAXWELL's equations acquired its full significance. For practical purposes at least, the question of the structure of the aether appeared quite superfluous: the electromagnetic concepts and equations formed a self-contained, consistent system, offering a firm starting point for the analysis of the new phenomena. There was, however, in this phenomenological point of view a quite novel element: as a result of HERTZ' discovery, the electromagnetic field had now become as concrete a reality as any dynamical system, and could serve as a complete substitute for the aether; MAXWELL's equations could be regarded as the expression for laws of nature as fundamental as those of dynamics. Thus, the old issue of the mechanical description of nature was not just pushed once more into the background: it was actually eliminated.

The way was then prepared for the emergence of the field concept as denoting a fundamental aspect of all natural phenomena. At the same time, the direct observation of atomic processes removed all doubts raised, at a different level of knowledge, about the existence of atoms. With the recognition of these two general features of the physical world, the fruitfulness of the great guiding ideas we saw at work in the course of this development exhausted itself. Both the dynamical philosophy, which had played such a decisive part in the elaboration of the field concept, and the mechanistic conception, achieving its greatest triumph in the atomistic interpretation of thermodynamics, became obsolete as a result of the broadening of outlook they had themselves brought about. As they vanished from the scene, they left unresolved the question whether the ultimate structure of matter and electricity was continuous or discontinuous. We know how deeply hidden the solution of this conflict was: a synthesis of the opposite aspects exhibited by all natural agencies could only be obtained from the recognition of the novel relationship of complementarity in which they stand to each other. Such a synthetical view (quite apart from the factual knowledge of the existence of the quantum of action, on which it is based) was undreamt of in the scientific philosophy of the XIXth century. Physics then was not ripe for any truly synthetic process: this was the pioneering epoch which prepared the elements for future syntheses. Hence this tendency we noticed to one-

sided generalisations and sharp oppositions, which gave the development of theoretical conceptions such a dramatic character. There is no more striking illustration of the tenseness of physical thought in that heroic period than LORENZ' and MAXWELL's struggle to establish the electromagnetic theory of light.

REFERENCES

[1] S. LAPLACE: *Exposition du système du monde* (Paris, 1796), 4th Ed. 1813; (livre IV, chap. XVIII: *De l'attraction moléculaire*).

[2] J. FOURIER: *Théorie analytique de la chaleur* (Paris, 1822).

[3] *a*) H. C. ØRSTED: *Experimenta circa effectum conflictus electrici in acum magneticum.* (Hafniae, 1820) (facsimile edition with translations by A. LARSEN, Copenhagen, 1920).

b) *Samlede og efterladte Skrifter* (Kjøbenhavn, 1852), vol. 9, p. 75.

[4] A. M. AMPÈRE: in *Collection de mémoires relatifs à la physique* (Paris, 1887).

a) (vol. III, p. 1) *Mémoire sur la théorie mathématique des phénomènes électrodynamiques, uniquement déduite de l'expérience* (1820-25).

b) (vol. II, pp. 249-251) (1822).

[5] W. WEBER: *Werke* (Berlin; vol. III, 1893; vol. IV, 1894).

a) (vol. III, p. 25) *Elektrodynamische Maassbestimmungen über ein allgemeines Grundgesetz der elektrischen Wirkung* (1846).

b) (vol. III, p. 609) (mit R. KOHLRAUSCH) *Elektrodynamische Maassbestimmungen insbesondere Zurückführung der Stromintensitäts-Messungen auf mechanisches Maass* (1957) [Announcement (vol. III, p. 591) on 20 October 1855].

c) (vol. IV, p. 105) *Elektrodynamische Maassbestimmungen, insbesondere über elektrische Schwingungen* (1864).

d) (vol. IV, p. 157) *ibid.*

[6] G. KIRCHHOFF: *Gesammelte Abhandlungen* (Leipzig, 1882).

a) (p. 131) *Über die Bewegung der Electricität in Drähten* (1857).

b) (p. 154) *Über die Bewegung der Electricität in Leitern* (1857).

[7] B. RIEMANN: *Gesammelte mathematische Werke*, 2nd Ed. (Leipzig, 1892).

a) (p. 553).

b) (p. 288) *Ein Beitrag zur Elektrodynamik* (1858, published 1867).

[8] L. LORENZ: *Oeuvres scientifiques* (revues et annotées par H. VALENTINER) (Copenhague, 1896).

a) (vol. I, p. 173) *Om Identiteten af Lyssvingninger og elektriske Strømme* (1867).

b) (vol. II, p. 3) *Mémoires sur la théorie de l'élasticité des corps homogènes à élasticité constante* (1861).

c) M. PIHL: *Der Physiker L. V. Lorenz* (Kopenhagen, 1939), p. 101.

[9] M. FARADAY: *Experimental researches in electricity* (London; vol. I, 1839; vol. II, 1844; vol. III, 1855).
a) (vol. III, p. 1).
b) (vol. III, p. 447) (1846).
c) (vol. II, p. 284) (1844).
[10] B. JONES: *The life and letters of Faraday* (London, 1869), vol. II, p. 378.
[11] S. THOMPSON: *Michael Faraday - his life and work* (London, 1898), p. 197.
[12] Sir W. THOMSON: *Reprint of papers on electrostatics and magnetism* (London, 1872).
a) (p. 1); *b*) (p. 126); *c*) (p. 15); *d*) (p. 419).
e) *Mathematical and physical papers* (London, 1882), vol. I, p. 76.
[13] S. THOMPSON: *The life of William Thomson* (London, 1910).
a) (vol. I, pp. 304-5); *b*) (vol. I, p. 311); *c*) (vol. I, p. 42); *d*) (vol. I, p. 56); *e*) (vol. I, p. 113); *f*) (vol. I, pp. 127, 128, 131); *g*) (vol. I, p. 197); *h*) (vol. I, p. 203); *i*) (vol. I, p. 524); *j*) (vol. II, p. 1024); *k*) (vol. II, pp. 835-6).
[14] J. C. MAXWELL: *Scientific papers* (Cambridge, 1890).
a) (vol. II, p. 360).
b) (vol. I, p. 155) *On Faraday's lines of force* (1855-6).
c) (vol. I, p. 451) *On physical lines of force* (1861-2).
d) (vol. I, p. 526) *A dynamical theory of the electromagnetic field* (1864).
e) (vol. I, pp. 570-1); *f*) (vol. I, p. 486); *g*) (vol. I, p. 582).
h) (vol. II, p. 125) *On a method of making a direct comparison of electrostatic with electromagnetic force; with a Note on the electromagnetic theory of light* (1868).
[15] J. C. MAXWELL: *A treatise on electricity and magnetism* (Oxford, 1873).
a) (Preface); *b*) (vol. II, § 831); *c*) (vol. II, chap. VII-VIII); *d*) (vol. I, § 105).
[16] L. CAMPBELL and V. GARNETT: *The life of James Clark Maxwell* (London, 1882; 2nd Ed., 1884).
a) (pp. 519-520); *b*) (pp. 288-90); *c*) (2nd Ed., p. 202); *d*) (p. 216); *e*) (2nd Ed., p. 243); *f*) (p. 340); *g*) (p. 342).
[17] H. HERTZ: *Gesammelte Werke* (Leipzig, 1894).
a) (vol. I, p. 339) *Über die Beziehungen zwischen Licht und Elektrizität* (1889).
b) (vol. III, p. xxiii); *c*) (vol. II, pp. 23-31).
d) (vol. I, p. 295) *Über die Beziehungen zwischen den Maxwellschen elektrodynamischen Grundgleichungen und den Grundgleichungen der gegnerischen Elektrodynamik* (1884).
e) (vol. II, p. 147) *Die Kräfte elektrischer Schwingungen, behandelt nach der Maxwellschen Theorie* (1888).
f) (vol. II, p. 208) *Über die Grundgleichungen der Elektrodynamik für ruhende Körper* (1890).
g) (vol. III) *Die Prinzipien der Mechanik in neuem Zusammenhange dargestellt* (1894).
[18] L. BOLTZMANN: *Populäre Schriften* (Leipzig, 1905), p. 227.
[19] E. T. WHITTAKER: *A history of the theories of aether and electricity* (London, 1910).
a) (p. 296); *b*) (p. 353 sqq.).
[20] L. ROSENFELD: *La première phase de l'évolution de la théorie des quanta*, in *Osiris*, **2**, 149 (1936).
[21] N. BOHR: *Drei Aufsätze über Spektren und Atombau* (Braunschweig, 1922): Erster Aufsatz (1913), p. 19.
[22] H. WEYL: *Raum-Zeit-Materie* (Berlin, 1919), 3rd Ed.
[23] D. VAN DANTZIG: *Proc. Cambr. Phil. Soc.*, **30**, 421 (1934); *Proc. Amst.*, **37**, 521, 526, 643, 825 (1934) and especially **39**, 3 (1936).

[24] *Correspondance entre Harald Høffding et Emile Meyerson* (Copenhague, 1939), p. 43.
[25] W. M. SMART: *John Couch Adams and the discovery of Neptune* (London, 1947).

Note. – P. DUHEM: *Les théories électriques de J. Clerk Maxwell* (Paris, 1902). This book is entirely devoted to a disparaging criticism of Maxwell's work. DUHEM discovers in particular a large number of « errors of sign » in Maxwell's papers, which according to him reveal serious inconsistencies in his ideas. It must be admitted that Maxwell's shifting choices of sign are rather confusing; there is even a definite inconsistency in the sign conventions of his 1864 paper, which gives rise to some obvious mistakes: but such formal blemishes do not affect the soundness of the physical argument.

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